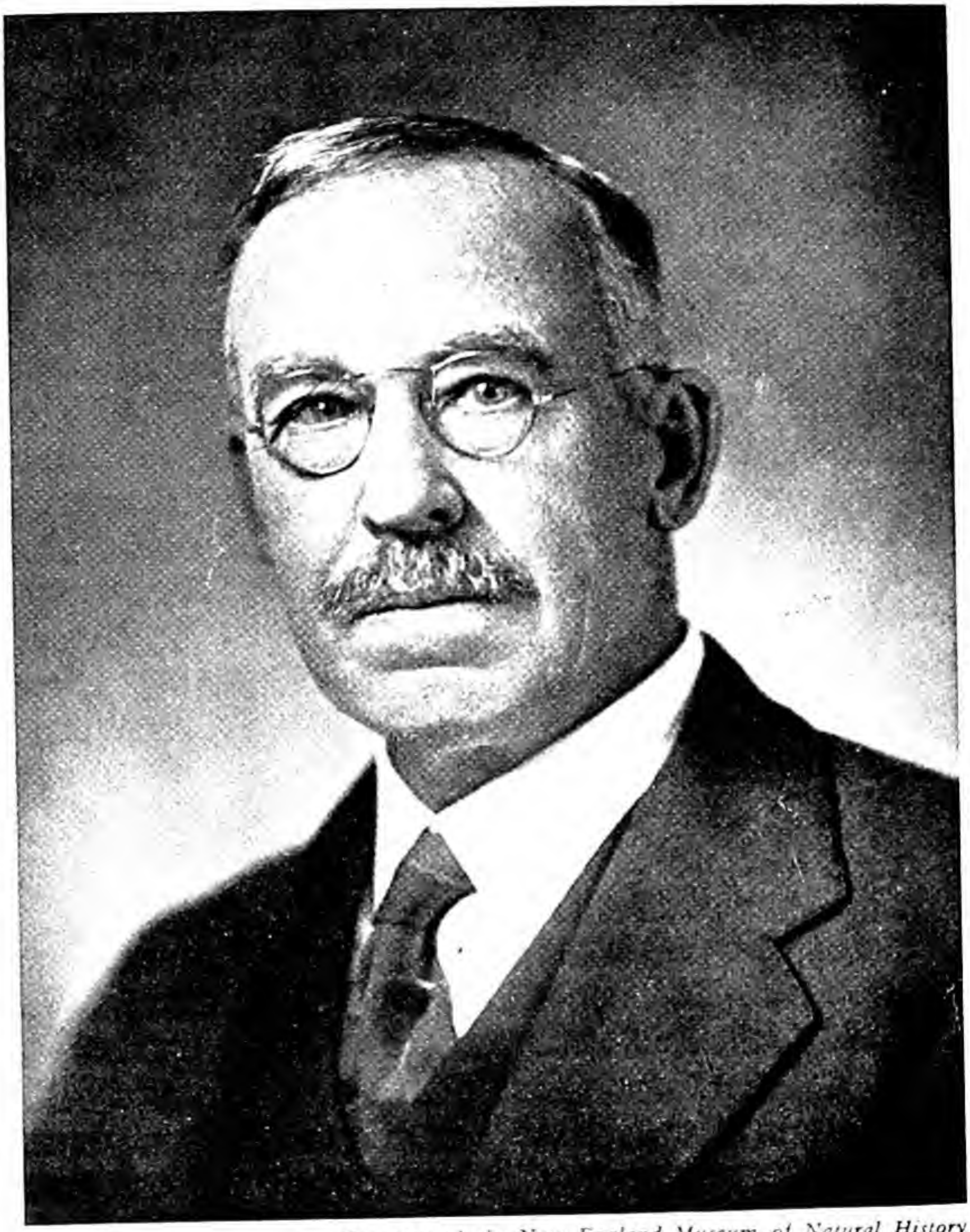
ESSAYS IN PHILOSOPHICAL BIOLOGY



Courtesy of the New England Museum of Natural History

WILLIAM MORTON WHEELER

ESSAYS IN PHILOSOPHICAL BIOLOGY

By

WILLIAM MORTON WHEELER

SELECTED BY PROFESSOR G. H. PARKER

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FOREWORD

I SUPPOSE that some who pick up this book will ask why these essays have been reprinted. The first reason is, of course, that most of them are out of print and there have been many requests for non-existent reprints. The second and much more important reason is the fact that these essays are utterly unique in the history of American biology. Unique because Wheeler was unique. No other naturalist born in this country, and but very few born elsewhere, was so versatile, so erudite and, at the same time, so marvelously gifted with a sense of humor which was pungent with gentle but very penetrating ridicule.

Wheeler wrote most of these essays in a spirit of fun and spent extraordinarily little time in their preparation. I do not think that he himself ever really rated them very highly for he was unbelievably modest and ill equipped to appraise his own intellectual products. In talking of himself, as he often did, for we lunched together no less than 447 times between 1930 and 1937, and how many times before that I have no record, he always made it clear that he considered his taxonomic and ecological work of much more importance than anything else which he did and he rated artistry in taxonomy above all other of his powers. No one who knew him, or his work, can deny that his systematical work will last as long as there are entomologists and insects; on the other hand still less could one deny that his vast range of varied reading, his penetrating knowledge of the classics, and his extraordinary familiarity with modern foreign

languages in their utmost refinements, finds an outlet and expression in these articles which, from the very deathlessness of their quality, demands preservation in a more permanent form than he gave them in their original publication.

Wheeler loathed pedantry, sham, and self-advertisement more than any other of the baser attributes of mankind. How well I remember his delight when he came back from his last trip to Australia with a new word which he often used thereafter. In addition to "flub-dub," which he used for these evil qualities, he now had the word "wowser" for their possessor, and the tone of his voice and the way the pupils of his eyes contracted to very pin points when he used these words showed how deep and how sincere was his abhorrence of what they, to his mind, conveyed.

It is really presumptuous to write an introduction to this little book. It needs none.

THOMAS BARBOUR

Museum of Comparative Zoölogy
Cambridge, Massachusetts
October, 1938

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WILLIAM MORTON WHEELER [1]

WILLIAM MORTON WHEELER was born at Milwaukee, Wisconsin, on March 19, 1865. He first attended public school but later transferred to Engelmann's German Academy and graduated from the German-American Normal School, which was appended to the academy. Even as a boy he was intensely interested in natural history and haunted the old museum at the school. In 1884, an incident occurred which was to influence his whole subsequent life. This was the visit to Milwaukee of Professor H. A. Ward, of Ward's Natural Science Establishment in Rochester. Ward brought with him a collection of stuffed and skeletonized mammals, birds, etc., with the idea of having the academy museum converted into a free municipal museum. Then a boy of nineteen years, Wheeler helped Professor Ward prepare the collection for exhibition and was offered, and promptly accepted, a position in the Rochester Establishment. His duties consisted of identifying, listing and arranging collections of birds, mammals, shells, echinoderms and sponges. The catalogue of shells which he then prepared is still used by conchologists. In the following spring (1885) he left Ward's and returned to Milwaukee, starting his career as a teacher. Dr. George W. Peckham, who had been making studies on spiders and on the behavior of wasps, induced him to accept a position as teacher of Ger-

[1] An appreciation signed by L. J. Henderson, Thomas Barbour, F. M. Carpenter, and Hans Zinsser, published in *Science*, June 4, 1937, and reprinted in the *Bulletin* of the New England Museum of Natural History for July 1937; included here by permission of The Science Press.

man and physiology in the Milwaukee High School, of which Peckham was principal. Within a very few years the Allis Lake Laboratory was established near the high school, and Professor C. O. Whitman was appointed its director. One of the assistants at the laboratory, Dr. William Patten, taught Wheeler the latest embryological technique and suggested that he investigate the embryology of insects. This resulted (1893) in the publication of Wheeler's *Contribution to Insect Embryology*, now recognized as a classic.

Meanwhile, however, the Milwaukee Public Museum had been established, and in 1887 Wheeler, at the age of twenty-two, was appointed its custodian. He held that position until 1890, when he accepted a fellowship at Clark University under C. O. Whitman. In 1892 Wheeler received his Ph.D. from Clark University, his dissertation being the embryological treatise previously mentioned. The following year (1893–94) he studied at Würzburg, at Liége and at the Naples Zoölogical Station. On his return to this country he was appointed instructor in embryology at the University of Chicago, and in 1896 was advanced to assistant professor. In 1899 he went to the University of Texas as professor of zoölogy. It was while there that he became especially interested in ants. Four years later (1903) he was selected as curator of invertebrate zoölogy at the American Museum of Natural History in New York. In 1908 he came to Harvard as professor of economic entomology. From 1915 until 1929 he was dean of the Bussey Institution, a graduate school of the university for research in applied biology. In the year 1924–1925 he was exchange professor at the University of Paris, and from 1926 until his retirement in 1933 he was professor of entomology and associate curator of insects at

the Museum of Comparative Zoölogy at Harvard. He died suddenly in Cambridge, Mass., on April 19, 1937, in his seventy-third year.

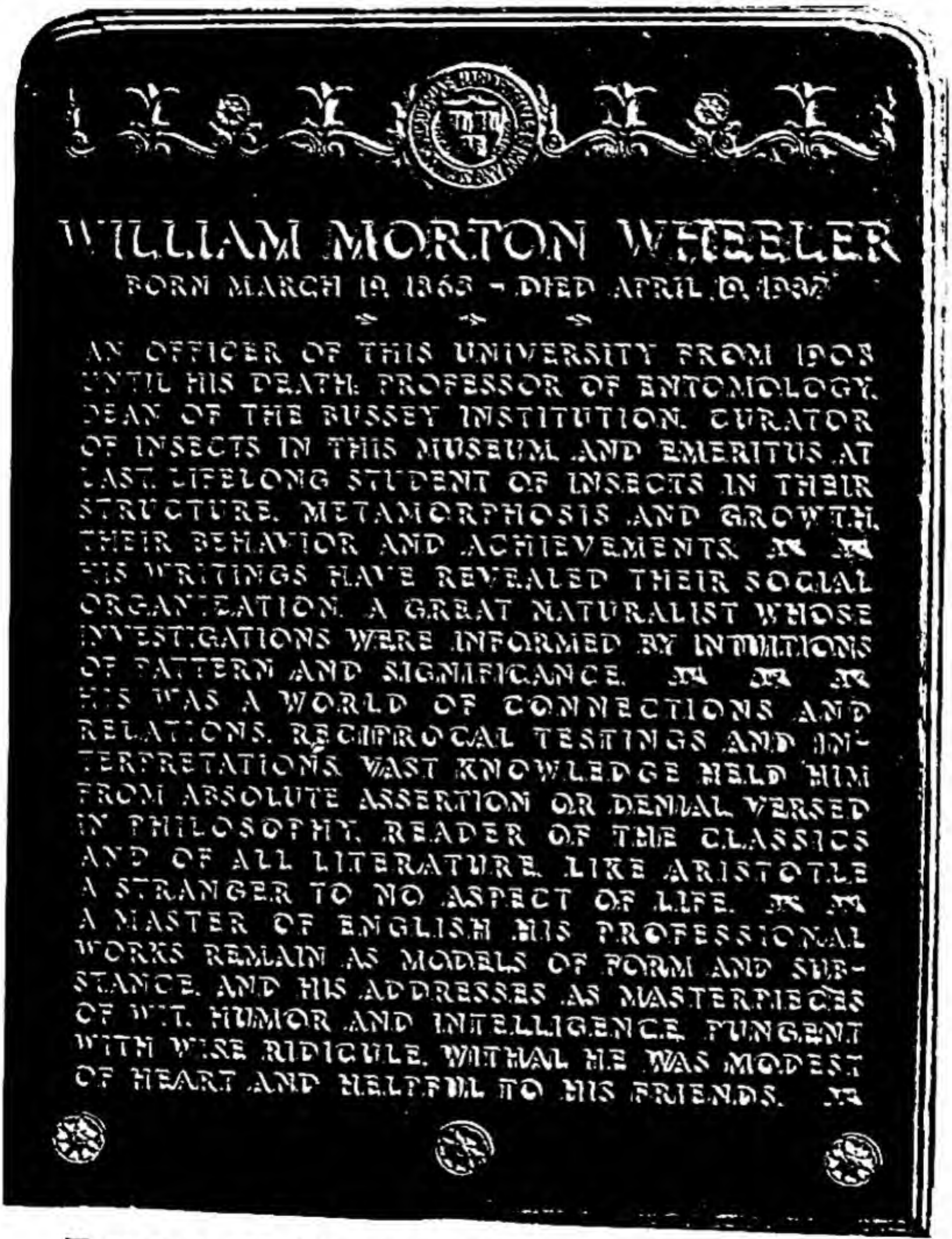
Professor Wheeler's bibliography contains 467 titles. Many of these papers are concerned with the classification, structure and behavior of ants, but a considerable number deal with problems of embryology, evolution, parasitism and the social life of insects in general. Several of these publications appeared in book form, the more notable ones being: *Ants, their Structure, Development and Behavior*; *The Social Insects, their Origin and Evolution*; *Foibles of Insects and Men*, and *Demons of the Dust, a Study of Insect Behavior.*

Wheeler had served his apprenticeship as a naturalist before his formal education in zoölogy really began, and this was perhaps the greatest good fortune of his life. Indeed many of his friends have always felt that the full development of his great qualities was in this way facilitated and assured. In thought and feeling he was a practitioner *and* a theorist; a specialist of the first rank, and, in the ancient sense, a philosopher; a great professor, a man of vast encyclopedic learning and the least pedantic of men; a diagnostician of genius who could instantly recognize the significant patterns in things and events, but who confirmed his conclusions by meticulous and systematic observation and study. He always had and never lost satisfaction in the pursuit of minute detail and in the accumulation of facts, so that hard work was a necessity of his being, but he could set no limits, within his wide competency, to the scope of his thought or to its sources or to its reference. All these traits, or at least their full development, he believed partly the result of his peculiar training, and he was in the habit of attributing

similar things to similar experiences in other cases than his own.

Observation of the social insects in the field led Wheeler — it could not fail to lead such a man, so conditioned and so oriented — to ecology, to psychology and to sociology. He worked long and hard at insect ecology, insect behavior and insect sociology, and in so doing found a most acceptable complement to his taxonomic work. But more than this, he made of himself a learned sociologist and psychologist and a master of the comparative branches of these sciences. It was such studies especially that directed his later thinking about evolution, that made him sceptical of the sufficiency of experimental evidence against the inheritance of acquired characters, that tempered his enthusiasm for the results and theories of the geneticists as a sufficient explanation of the mechanism of evolution, and that won his sympathy, warm though qualified, for the theory of emergent evolution. These studies also chiefly determined his philosophical position, so different from that of many of those who base their position on mathematics and the physical sciences. Wheeler, like the great physicians, could not forget the inconceivable complexity of things as they are and the intricacy of the web of events, but he possessed that intuitive and imaginative understanding which is the naturalist's compensation for his lack of the clear analysis of the physicist. Wheeler's philosophical position was, accordingly, chiefly the result of a naturalist's disciplined imagination and of vast first-hand acquaintance with animals and their behavior.

He was a man of letters. Possibly the most widely read member of his university, and in this respect unique among the men of science, he was also a distinguished prose writer.



TABLET IN THE MUSEUM OF COMPARATIVE ZOÖLOGY AT HARVARD COLLEGE

Both facts were, or seemed to those who knew him, very certainly and deeply characteristic, nothing less than necessary expressions of his personality.

His reading was limited only in the intellectual sphere by a disposition to avoid the more abstract sciences and, perhaps, in matters of taste by other less important preferences. It included the literature of many languages both ancient and modern and everything that he thought possibly of even the smallest interest as an addition to his accumulated store of knowledge and experience. His writing was the expression of his sensitive feeling for style and of his ideal of good workmanship. At its best, for instance, in his occasional satirical pieces, like the letter from the king of the termites, and in "The Dry-Rot of our Academic Biology," it has a force and a polish, not to mention other qualities, that recall Voltaire.

One can appraise the contributions which an unusual man has made to the civilization of his time. It is almost impossible, however, to convey in words the personality compounded of intellectual and spiritual qualities which characterized the individual as a whole and lent him his flavor and charm. It is quite certain that Wheeler never thought of himself as a great man. In so many ways he was the superior of those about him and his learning and originality were so freely acknowledged that a certain amount of the conceit not uncommon in lesser men might have been excusable. To some extent his sense of humor saved him from this. Like all really great men, he was extraordinarily good company. He laughed with one and, inoffensively, at one; and he was one of the very rare individuals whose idiomatic knowledge of three or four languages was such

that he could laugh with equal gusto in all of them. During his later years, he spent most of his evenings in his study in West Cedar Street, where one would find him sitting at a deskful of books — with more books on chairs and on the floor and with sheets of manuscript scattered under and over them. The casual visitor was installed in an armchair and the maid sent down for a bottle and the cigars. He had always read some book that other people read later — often at his instigation. His conversation would pick up from this or from some reminiscence that might lead in almost any direction from classical literature to recent discoveries of science. It was difficult to find anything of importance that he had not read — and the scope of his readings ranged from Wilhelm Busch and Alice in Wonderland to Whitehead, who himself regarded Wheeler as one of the greatest men he had ever met. A student wrote the following to Mrs. Wheeler: "In a recent lecture, Professor Whitehead characterized him as the only man he had ever known who would have been both worthy and able to sustain a conversation with Aristotle."

A highly developed specialist in his own calling, Wheeler was more completely the intellectual man of the world than any but a very few of his contemporaries in this or any other country. One never left him without having learned something, and one walked down the hill after an evening with him with ever-renewed admiration and affection — and usually with a chuckle.

The death of a great naturalist, like that of a great physician, does more than put an end to a scientific career. It destroys an accumulation and synthesis of knowledge, skill, judgment and experience that cannot be transmitted and

preserved, because it is as yet incommunicable. To some of Wheeler's friends and colleagues these things seemed the best part of what by devotion, industry, enthusiasm and high intelligence he had made of himself professionally, an achievement even greater than his contributions to science and never to be replaced.

His written contributions to his subject will perpetuate his scientific memory, and his less technical writings will be read with interest and amusement for a long time to come. But as a personality, Wheeler was one of the great experiences in the lives of his friends and, in this sense, he will not really die until all those who knew him well are gone.

ESSAYS IN PHILOSOPHICAL BIOLOGY

I

THE ANT-COLONY AS AN ORGANISM [1]

Une autre hypothèse pourrait considérer la ruche, la fourmilière et la termitière comme un individu unique, mais encore ou déjà disseminé, un seul être vivant qui ne serait pas encore ou qui ne serait déjà plus coagulé ou solidifié et dont les divers organes, formés de milliers de cellules, bien qu'extériorisées et malgré leur apparente indépendance resteraient toujours soumis à la même loi centrale. Notre corps aussi est une association, un agglomerat, une colonie de soixante trillions de cellules, mais de cellules qui ne peuvent pas s'éloigner de leur nid, ou de leur noyau, et demeurent, jusqu'à la destruction de ce nid ou de ce noyau, sédentaires et captives. Si terrible, si inhumaine que paraisse l'organisation de la termitière, celle que nous portons en nous est calquée sur le même modèle. Même personalité collective, même sacrifice incessant d'innombrables parties au tout, au bien commun, même systeme defensif, même cannibalisme de phagocytes envers les cellules mortes ou inutiles, même travail obscur, acharné, aveugle, pour une fin ignorée, même ferocité, mêmes spécialisations pour la nutrition, la reproduction, la respiration, la circulation du sang, etc., mêmes complications, même solidarité, mêmes appels en cas de danger, mêmes équilibres, même police intérieure. — MAETERLINCK, *La Vie des Termites*

AS A ZOOLOGIST, reared among what are now rapidly coming to be regarded as antiquated ideals, I confess to a feeling of great diffidence in addressing an audience so thoroughly versed in the very latest as well as the very oldest biological facts, methods, and hypotheses. I feel, indeed, like some village potter who is bringing to the market of the metropolis a pitiable sample of his craft, a pot of some old-fashioned design, possibly with a concealed crack which

[1] A lecture prepared for delivery at the Marine Biological Laboratory, Woods Hole, Mass., August 2, 1910, and published in the *Journal of Morphology*, vol. XXII (Boston, 1911) and in *Foibles of Insects and Men* (New York: A. A. Knopf, 1928); included here by permission of the Wistar Institute of Anatomy and Biology and A. A. Knopf.

may prevent it from ringing true. Although in what I have to say, I shall strenuously endeavor to be modern, I can only beg you, if I fail to come within hailing distance of the advance guard of present-day zoologists, to remember that the range of adaptability in all organisms, even in zoologists, is very limited.

Under the circumstances, my only hope lies in appealing to our permanent common biological interests, and these, I take it, must always center in the organism. But the point of view from which we study this most extraordinary of nature's manifestations is continually shifting. Twenty years ago we were captivated by the morphology of the organism, now its behavior occupies the foreground of our attention. Once we thought we were seriously studying biology when we were scrutinizing paraffine sections of animals and plants or dried specimens mounted on pins or pressed between layers of blotting paper; now we are sure that we were studying merely the exuviae of organisms, the effete residua of the life-process. If the neovitalistic school has done nothing else, it has jolted us out of this delusion which was gradually taking possession of our faculties. It is certain that whatever changes may overtake biology in the future, we must henceforth grapple with the organism as a dynamic agency acting in a very complex and unstable environment. In using the term organism, therefore, I shall drop the adjective "living," since I do not regard pickled animals or dried plants as organisms.

As I wish to describe a peculiar type of organism, I may be asked, before proceeding, to state more concisely what I mean by an organism. It is obvious that no adequate definition can be given, because the organism is neither a

thing nor a concept, but a continual flux or process, and hence forever changing and never completed. As good a formal definition as I can frame is the following: An organism is a complex, definitely coördinated and therefore individualized system of activities, which are primarily directed to obtaining and assimilating substances from an environment, to producing other similar systems, known as offspring, and to protecting the system itself and usually also its offspring from disturbances emanating from the environment. The three fundamental activities enumerated in this definition, namely nutrition, reproduction, and protection, seem to have their inception in what we know, from exclusively subjective experience, as feelings of hunger, affection, and fear, respectively.

Biologists long ago constructed an elaborate hierarchy of organisms. Those of a speculative turn of mind, like Spencer and Weismann, postulated the existence of very simple organisms, the physiological units, or biophores, which, though invisible, were nevertheless conceived as combining the fundamental activities above enumerated. These biophores were supposed to form by aggregation the cells, which may exist as independent organisms in the Protozoa and Protophyta or unite with other cells to form more complex aggregates, for which Haeckel's term "persons" may be adopted. The person may be merely a cell-aggregate or consist of complexes of such aggregates as the metameres of the higher animals, for the separate metameres, according to a very generally accepted theory, are supposed to be more or less modified or highly specialized persons. Somewhat similar conditions are supposed to obtain in the composition of the vascular plants. The integration both of the meta-

meric and non-metameric Metazoa may proceed still further, the simple persons combining to form colonies in which the persons are primarily nutritive and acquire fixed and definite spatial relations to one another, whereas the more specialized animals, like the social insects, may constitute families of mobile persons with reproduction as the *Leitmotiv* of their consociation. In man we have families associating to form still more complex aggregates, the true societies. Other comprehensive organisms are the cœnobioses, or more or less definite consociations of animals and plants of different species, which the ecologists are endeavoring to analyze. Finally, we have philosophers, like Fechner, stepping in with the assertion that the earth as a whole is merely a great organism, that the planetary systems in turn are colonies of earths and suns, and that the universe itself is to be regarded as one stupendous organism. Thus, starting with the biophore as the smallest and ending with the universe as the most comprehensive, we have a sufficiently magnificent hierarchy of organisms to satisfy even the most zealous panpsychist. As biologists we may, for present purposes, lop off and discard the ends of this series of organisms, the biophores, as being purely hypothetical and the cosmos as involving too many ultrabiological assumptions. We then have left the following series: first, the Protozoon or Protophyte, second the simple or non-metameric person, third the metameric person, fourth the colony of the nutritive type, fifth the family, or colony of the reproductive type, sixth the cœnobiose, and seventh the true, or human, society. Closer inspection shows that these are sufficiently heterogeneous when compared with one another and with the personal organism, which is the prototype of the series, but I believe, nevertheless, that all of them

are real organisms and not merely conceptual constructions or analogies. One of them, the insect colony, has interested me exceedingly, and as I have repeatedly found its treatment as an organism to yield fruitful results in my studies, I have acquired the conviction that our biological theories must remain inadequate so long as we confine ourselves to the study of the cells and persons and leave the psychologists, sociologists, and metaphysicians to deal with the more complex organisms. Indeed our failure to coöperate with these investigators in the study of animal and plant societies has blinded us to many aspects of the cellular and personal activities with which we are constantly dealing. This failure, moreover, is largely responsible for our fear of the psychological and the metaphysical, a fear which becomes the more ludicrous from the fact that even our so-called "exact" sciences smell to heaven with the rankest kind of materialistic metaphysics.[2]

Leaving these generalities for the present, permit me to present the evidence for the contention that the animal colony is a true organism and not merely the analogue of the person. To make this evidence as concrete as possible I shall take the ant-colony as a paradigm and ask you to accept my statement that the colonies of the termites, social bees, and wasps, which the limited time at my disposal does not permit us to consider, will be found to offer the same and in some cases even more satisfactory data. I select the ant-colony not only because I am more familiar with its activities, but because it is much more interesting than that of the

[2] The hierarchy of organisms briefly considered in this paragraph now finds its place in "emergent evolution," which is discussed in my *Emergent Evolution and the Development of Societies* (New York: W. W. Norton, 1928). [Reprinted in part below, page 143 and following.]

polyps, more typical and less specialized than that of the honey bee, less generalized than that of the wasps and bumble-bees, and has been much more thoroughly investigated than the colonies of the stingless bees and the termites.

The most general organismal character of the ant-colony is its individuality. Like the cell or the person, it behaves as a unitary whole, maintaining its identity in space, resisting dissolution, and, as a general rule, any fusion with other colonies of the same or alien species. This resistance is very strongly manifested in the fierce defensive and offensive cooperation of the colonial personnel. Moreover, every ant-colony has its own peculiar idiosyncrasies of composition and behavior. This is most clearly seen in the character of the nest, which bears about the same relation to the colony that the shell bears to the individual Foraminifer or mollusk. The nest is a unitary structure, built on a definite but plastic design and through the coöperation of a number of persons. It not only reflects the idiosyncrasies of these persons individually and as a whole, but it often has a most interesting adaptive growth and orientation which may be regarded as a kind of tropism. In many species the nest mounds, which are used as incubators of the brood and as sun-parlors for the adult ants, are constructed in such a manner as to utilize the solar radiation to the utmost. In the Alps and Rocky Mountains we find the nests oriented in such a manner that the portions in which the brood is reared face south or east, and as time goes on the nests often grow slowly in these directions, like plants turning to the light, so that they become greatly elongated. This orientation is, in fact, so constant in some species that the Swiss mountaineers, when lost in a fog, can use it as a compass.

Every complete ant-colony, moreover, has a definite stature which depends, of course, on the number of its component persons. And this stature, like that of personal organisms, varies greatly with the species and is not determined exclusively by the amount of food but also by the queen mother's fertility, which is constitutional. Certain ants live in affluence but are nevertheless unable to form colonies of more than fifty or a hundred individuals, while others, under the same conditions, have a personnel of thousands or tens of thousands.

One of the most general structural peculiarities of the person is the duality of its composition as expressed in the germ-plasm on the one hand and the soma (body) on the other, and the same is true of the ant-colony, in which the mother queen and the virgin males and females represent the germ-plasm, or, more accurately speaking, the *Keimbahn*, while the normally sterile females, or workers and soldiers, in all their developmental stages, represent the soma. In discussing the question of the inheritance or non-inheritance of acquired characters the Neo-Darwinians trace all the congenital modifications of the worker and soldier phases to the queen, just as in the personal organism all the congenital somatic characters are traced to the germ-plasm of the egg. Since the homologue of the reproductive organ of the ant-colony consists of the virgin males and females, and since the males mature earlier than the females, the colony may be regarded as a protandric hermaphrodite. Some colonies, however — and this is probably characteristic of certain species — produce only males or females and are therefore in a sense gonochoristic, or diœcious. And this protandric hermaphroditism and gonochorism, like the corresponding

conditions in persons, may be interpreted as a device for, or, at any rate, as an aid in insuring cross-fertilization. The fecundated queen of the ant-colony represents the first link in the *Keimbahn* and therefore corresponds to the fertilized egg of the personal organism. She produces both the worker personnel and the virgin males and females, just as the fertilized egg produces both the soma and the germ-cells. The colonial soma, moreover, may be differentiated as the result of a physiological division of labor into two distinct castes, comprising the workers, in which the nutritive and nidificational activities predominate, and the soldiers, which are primarily protective. Here, too, the resemblance to the differentiation of the personal soma into entodermal and ectodermal tissues can hardly be overlooked.

The structure of the ant-colony thus appears to be very simple as compared with that of its component persons. The question naturally arises as to the particular type of unicellular or personal organism which it most resembles. Undoubtedly, if we could see it acting in its entirety, the ant-colony would resemble a gigantic foraminiferous Rhizopod, in which the nest would represent the shell, the queen the nucleus, the mass of ants the plasmodium, and the files of workers, which are continually going in and out of the nest, the pseudopodia.

The ant-colony, of course, like the person, has both an ontogenetic and a phylogenetic development; the former open to observation, the latter inferred from the ontogeny, a comparison of the various species of ants with one another and with allied Hymenopterous insects, and from the paleontological record. The fecundated queen, as I have stated, represents the fertilized egg which produces the colonial

organism, but she is a winged and possibly conscious egg, capable not only of actively disseminating the species, like the minute eggs of many marine animals, but of selecting the site for the future colony. After finding this site she discards her wings and henceforth becomes sedentary like the wingless workers which she will produce. The whole colony rests satisfied with the nesting site selected by its queen if the environmental conditions remain relatively constant. If these become unfavorable, however, the colony will move as a whole to a new site. In most species such movements are rather limited, but the nomadic driver and legionary ants are almost continually moving from place to place and must cover a considerable territory during the year. After the queen has selected the nesting site, she immures herself in some earthen or vegetable cavity, lays a number of eggs, supplying them with yolk derived by metabolism from her fat-body and now useless wing-muscles, and feeds the hatching larvæ on her salivary secretion, which, though highly nutritious, is, nevertheless, very limited in quantity, so that the offspring when mature are dwarfed and very few in number.[3] They are, in fact, workers of the smallest and feeblest caste; but they set to work enlarging the nest, break through the soil or plant tissues,

[3] This statement must now be modified, since E. Meyer has shown ("Die Ernährung der Mutterameise und ihrer Brut während der solitären Koloniegründung," *Biologisches Zentralblatt*, XLVII, 264–307, Leipzig, 1927) that the colony-founding queens of ants frequently and perhaps as a rule use their own eggs instead of their saliva as food for their first brood of larvæ. This was observed in several European ants (*Messor structor, Tetramorium semilaeve, Formica cinerea*, etc.). It has been previously noticed by J. Huber in the Brazilian fungus-grower, *Atta sexdens* ("Ueber die Koloniegründung bei Atta sexdens," *Biologisches Centralblatt*, XXV, 606–619, 625–635, Leipzig, 1905).

construct an entrance on the surface, and seek food for themselves and their famished mother. This food enables her to replenish her fat-body and to produce more eggs. Her expansive instincts and activities now contract, so to speak, and become reduced henceforth to a perpetual routine of assimilation, metabolism, and oviposition. She produces brood after brood during her long life, which may extend over a period of ten to thirteen years. Her workers assume the duties of foraging, of feeding the larvæ and one another, and of completing the nest. Their size and polymorphism increase with successive broods, till the soldier forms, if these are characteristic of the species, make their appearance. Then the individuals which correspond to the reproductive cells of the personal organism, namely the virgin males and females, develop, and the colonial organism may be said to have reached maturity. Like the personal organism, it may persist for thirty or forty years, or perhaps even longer, without much growth of its soma, since the workers and soldiers of which this consists are exposed to many vicissitudes and live only from three to four years and probably, as a rule, for a much shorter period. If the queen grows too old or dies, the colony as a rule dwindles and eventually perishes, unless her place is taken by one or more of her fertile daughters.

This is the ontogenetic history of most ant-colonies. It is so similar to the phylogenetic history derived from the sources mentioned above that we have no hesitation in affirming that it conforms in the most striking manner to the biogenetic law. The very ancient behavior of the solitary female Hymenopteron is still reproduced during the incipient stage of colony formation, just as the unicellular phase

of the Metazoon is represented by the egg. A further correspondence of the ontogeny and phylogeny is indicated by the fact that the most archaic and primitive of living ants form small colonies of monomorphic workers closely resembling the queen, whereas the more recent and most highly specialized ants produce large colonies of workers not only very unlike the queen but unlike one another.

In order to complete the foregoing account it will be necessary to consider some interesting modifications of the usual method of colony formation and growth, especially as these modifications furnish additional and striking evidence in favor of the contention that the ant-colony is a true organism. In many species, after the colony has reached maturity, and especially if the food-supply continues to be abundant, several of the virgin females may be fecundated in the nest, lose their wings, and remain as members of the colony. This may, indeed, contain half a dozen and in extreme cases as many as forty or fifty or even more fertile queens. But often the growth of the colonial organism becomes excessive through an increase in the worker personnel and passes over into a form of colonial reproduction, when the young fertilized queens, each accompanied by a band of workers, start new nests in the vicinity of the parental formicary. In this manner a very large and complex colony may arise and extend over many adjacent nests. For some time the new settlements may remain in communication with the home-nest through files of workers, but eventually the daughter settlements may become detached and form independent colonies. The resemblance of this method of reproduction, which is essentially the same as the swarming in the honey-bee, to the asexual reproduction of many unicellular and

multicellular organisms by a process of budding, is too obvious to need further comment.

The important role of nutrition in the development of the colony will be clear from the foregoing remarks. It becomes even more striking in the methods adopted by the queens of certain parasitic species in starting their colonies. Some European observers and myself have found a number of queen ants that are unable to found colonies without the aid of workers of allied species. These queens may be separated into four groups, as follows:

1. The queen that enters a colony of an alien species and decapitates its queen or is the occasion of her being killed off by her own workers. The intrusive queen is then adopted by the workers and a compound colonial organism arises, consisting of the germ-plasm of one species and the soma of another. The queen proceeds to lay eggs, which are reared by the alien workers, thus relieving her of all the labor and exhaustion endured by the independent typical ant queen during the early stages of colony formation. *Pari passu* with the development of the worker offspring of the intrusive queen, the worker nurses grow old and die, so that the colony eventually comes to consist of only one species, the soma of the host being replaced by that of the parasite. This method of colony formation, first observed among our American ants and later among certain European and North African species, I have called temporary social parasitism. Now many of the species that behave in this manner have extremely small queens, or queens provided with a peculiar pilosity or sculpture that tend to endear them to the workers of the alien colonies which they invade. If we regard the large fertilized queens of ordinary ants, which

are suppplied with a voluminous fat-body and wing-musculature, as representing eggs provided with a great amount of yolk, and the diminutive queens of the temporary social parasites as the equivalents of alecithal eggs, we have another striking resemblance between the personal and colonial organisms; for the large queens, like the yolk-laden eggs of many vertebrates, are produced in small numbers but are able to generate the colonial soma independently, whereas the small queens, which are produced in great numbers in order that some of them may survive the vicissitudes of a parasitic life, correspond to the small yolk-less eggs of many parasites, which have to be deposited in plant or animal tissues in order that the imperfect young on hatching may be surrounded by an abundance of food.

2. The queen of the blood-red slave-maker (*Formica sanguinea*) adopts a different method. She enters the colony of an allied species, snatches up the worker brood, and kills any of the workers or queens that endeavor to dispute her possession. The ants hatch with a sense of affiliation with their foster mother and proceed to rear her eggs and larvæ as soon as they appear. Here, too, the colony is formed by a mixture of two species, but the workers produced by the intrusive queen inherit her predatory instincts and therefore become slave-makers. They keep on kidnapping worker larvæ and pupæ from the nests of the alien species, carry them home, and eat some of them, but permit many to mature, so that the mixed character of the colony is maintained. This, however, is not invariably the case, for old and vigorous *sanguinea* colonies may cease to make slave raids, and the slaves may die off and leave a pure colony of the predatory species. The advantages of this method of colony

formation are obvious, for the colonial soma, being composed of two species, grows more rapidly and is much more efficient as a nutritive and protective support to the colonial germ-plasm, which is restricted to the predatory species.

3. The colony-founding queen of the amazon ants of the genus Polyergus resorts to a modification of the method adopted by *sanguinea*, as has been shown by Emery's recent observations. She enters the colony of an alien species, perforates its queen's head with her sickle-shaped mandibles, and permits herself to be adopted by the workers. She pays no attention to the brood but begins to lay eggs, the larvæ from which are carefully reared by the workers. The Polyergus offspring inherit the pugnacity of their mother, but, like the *sanguinea* workers, have the ability to kidnap the brood of other ants. They are, in fact, slave-makers of a very deft and ferocious type. Like their mother, however, they are unable to excavate the nest, to care for their own young, or to take food except from the mouths of the workers that hatch from the kidnapped larvæ and pupæ. The mixture of the two species is therefore obligatory, and the slave personnel, which represents the nutritive and nest-building portions of the colonial soma, has to be maintained throughout the life of the colony.

4. Certain feeble queen ants belonging to a few aberrant genera (Anergates, Wheeleriella) invade populous nests of an alien species and are adopted in the place of their queens, which are destroyed by their own workers. The parasites then proceed to lay eggs but these give rise only to males and females as the worker caste is entirely suppressed. The colony retains a mixed character, the parasitic species usurping the functions of the germ-plasm, while the host is purely

somatic. As there are no means of prolonging the lives of the host workers and as they do not reproduce, the whole colony is short-lived and the maturation of the parasitic sexual individuals has to be accelerated so that it will fall within the brief lifetime of the worker hosts. This condition I have called permanent social parasitism.

These four peculiar types of colony formation all lead to the formation of compound colonial organisms, comparable to certain compound personal organisms which, with few exceptions, can be produced only by artificial means. In temporary social parasitism the colonial egg can develop its soma only when grafted on to the soma of another species. This soma eventually perishes, and the colony then assumes a normal complexion. This condition reminds us of certain tropical plants, like the species of Clusia and Ficus, which develop as epiphytes on other trees but after killing their hosts take root in the soil and thenceforth grow as independent organisms. The slave-makers of the *sanguinea* or facultative type are also unable to develop the soma except when grafted on to the soma of another species, but in this case the coöperation of both somas in nourishing and protecting the germ-plasm is maintained for a much longer period. This kind of colony may be compared with a graft made by uniting the longitudinal half of one plant with that of another so that both take nourishment through their roots. To make the resemblance more complete one of the grafted halves would have to be pruned in such a manner as to prevent flowering. In the amazons or obligatory slave-makers and the permanent social parasites the alien soma alone has a nutritive function, so that the conditions are like those in ordinary vegetable grafts, in which the stock re-

tains the roots and the scion produces the flowers and fruit.

I have dwelt on the various methods of colony formation not only because they give us an insight into colonial reproduction, but because they throw light on the colonial organism from the standpoint of parasitology. That the four types of queens and their offspring are directly comparable with entoparasitic persons is not so remarkable as the fact that in ants the host and parasite form a mixed organism which could only be obtained with persons by jumbling together the component cells of host and parasite like two kinds of peas shaken in a bottle. Notwithstanding this mixture the parasitic colony not only retains its identity and the anticipatory character of its behavior but castrates the host colony and constrains its soma either to coöperate in many of its activities or to specialize as a purely nutritive or nest-building auxiliary. The host is thus reduced to the status of a nourishing or protective organ of the parasite. This behavior has many striking analogies among persons. Giard long ago called attention to the fact that when the cirriped Sacculina settles under the abdomen of a male crab and sends its root-like haustoria into the tissues of its host the latter undergoes castration and its narrow abdomen expands to form a protection for the soft-bodied parasite. In other words, the parasite acts as if it were a mass of crabs' eggs, and the male crab behaves as if it had changed its sex and develops an abdomen of the female type.

Not only are there ants, like those already considered, that may be regarded as colonial entoparasites, but there are also a number of species that may be called colonial ectoparasites. These form the so-called "compound nests," in which two

or more species live amicably side by side, or may even mingle freely with one another, but rear their broods in separate nests, thus indicating in the clearest manner the integrity of the colonial organism. This is also shown by the vast number of myrmecophilous insects, which are, of course, ento- or ectoparasitic persons, and behave towards the ant-colony as if it were a rather incoherent and therefore more vulnerable or exploitable personal organism.

Finally we come to what the neovitalists regard as the most striking autonomic manifestations of the organism, namely the regulations and restitutions, and face the question as to whether these, too, have their counterpart in the colonial organism. I believe that the following facts compel us to answer this question in the affirmative. If the worker personnel be removed from a young ant-colony, leaving only the fertile queen, we find that this insect, if provided with a sufficiently voluminous fat-body, will set to work and rear another brood, or, in other words, regenerate the missing soma. And, of course, any portion of the worker or sexual personnel that is removed from a vigorous colony will be readily replaced by development of a corresponding portion of the brood. On the other hand, if the queen alone be removed, one of the workers will often develop its ovaries and take on the egg-laying function of the queen. In ants such substitution queens, or gynaecoid workers, are not fertilized and are therefore unable to assume their mother's worker- and queen-producing functions. The termites, however, show a remarkable provision for restituting both of the fertile parents of the colony from the so-called complemental males and females. In ants we have a production of fertile from normally infertile individuals, but the incompleteness

of the result does not disprove the existence of a pronounced restitutional tendency.

Very striking examples of this tendency are exhibited when colonies are injured by parasitic myrmecophiles. I shall consider only the case of the peculiar beetle *Lomechusa strumosa*, which breeds in colonies of the blood-red slave-maker (*Formica sanguinea*). Though the beetle and its larvæ are treated with great affection, the latter devour the ant larvæ in great numbers, so that little of the brood survives during the early summer months when the colony is producing its greatest annual increment to the worker personnel. The ants seem to perceive this defect and endeavor to remedy it by converting all the surviving queen larvæ into workers. But as these larvæ have passed the stage in their development when such an operation can be successful, the result is the production of a lot of pseudogynes, or abortive creatures structurally intermediate between the workers and queens and therefore useless in either capacity. It is instructive to compare this case with the regeneration of the lens from the iris in the Amphibian eye. In his recent analysis of the stimuli of restitution in personal organisms Driesch reaches the conclusion that "the specificity of what is taken away certainly forms part of the stimulus we are searching for, and it does so by being communicated in some way by something that has relations to *many*, if not *all*, parts of the organism and not only to the neighboring ones." He also says that "each part of the organism assigns its specific share to an unknown something and that this something is altered as soon as a part is *removed* or absolutely *stopped* in its functional life, and that the specific alteration of the something is our stimulus of restitutions." These quotations

and Driesch's further discussion of the problem are even clearer in their application to the colonial than to the personal organism, for in the former it is much easier to see how each individual insect "can do more than one thing in the service of restitution" than it is to understand how each cell of the person can do more than one thing in restoring a lost organ.

I fear that I may have wearied you with this long attempt to prove that the ant-colony is a true organism, especially as this statement must seem to some of you to be too trite for discussion, but when an author like Driesch writes a large work in two volumes on the "Philosophy of the Organism" and ignores the colonial organisms altogether, an old-fashioned zoologist may perhaps be pardoned for calling attention to a well-founded, though somewhat threadbare, biological conception.

If it be granted that the ant-colony and those of the other social insects are organisms, we are still confronted with the formidable question as to what regulates the anticipatory coöperation, or synergy of the colonial personnel and determines its unitary and individualized course. The resemblance of the ant- or bee-colony to the human state long ago suggested a naive reply to this question. Aristotle naturally supposed the colonial activities to be directed and regulated by a *βασιλεύς* or *ἡγεμών*, because these personages managed affairs in the Greek states. After the sex of the fertile individual had been discovered by Swammerdam, the word "queen" was naturally substituted for *βασιλεύς* or "king," and as queens in human states do not necessarily govern and are often rather anabolic, sedentary, and prolific persons, and the objects of much flattering attention, the

term is not altogether inapt when applied to the fertile females of insect colonies. It has been retained, although everybody knows that these colonies represent a form of society very different from our own, a kind of communistic anarchy, in which there is "neither guide, overseer, nor ruler," as Solomon correctly observed. In this respect, too, the colony is essentially the same as the personal organism, at least in the opinion of those who do not feel compelled to assume the existence of a "soul" in the scholastic sense. For it is clear that to primitive thinkers the soul was supposed to bear the same relation to the person as the βασιλεύς to the insect colony and the king to the human state. This supposition is still held, though in a more subtle form, by writers of the present day. Some of these, like Maeterlinck, clothe the postulated controlling agency in a mystical or poetic garb and call it the "spirit of the hive." The following passage from the Belgian poet's charming account of the honey-bee will serve to illustrate this method of meeting the problem:

> What is this "spirit of the hive" — where does it reside? It is not like the special instinct that teaches the bird to construct its well planned nest, and then seek other skies when the day for migration returns. Nor is it a kind of mechanical habit of the race, or blind craving for life, that will fling the bees upon any wild hazard the moment an unforeseen event shall derange the accustomed order of phenomena. On the contrary, be the event never so masterful, the "spirit of the hive" still will follow it, step by step, like an alert and quickwitted slave, who is able to derive advantage even from his master's most dangerous orders.
>
> It disposes pitilessly of the wealth and the happiness, the liberty and life, of all this winged people; and yet with discretion, as though governed itself by some great duty. It regulates day by day the number of births, and contrives that these shall strictly accord with the number of flowers that brighten the country-

side. It decrees the queen's deposition or warns her that she must depart; it compels her to bring her own rivals into the world, and rears them royally, protecting them from their mother's political hatred. So, too, in accordance with the generosity of the flowers, the age of the spring, and the probable dangers of the nuptial flight will it permit or forbid the first-born of the virgin princesses to slay in their cradles her younger sisters, who are singing the song of the queens. At other times, when the season wanes, and flowery hours grow shorter, it will command the workers themselves to slaughter the whole imperial brood, that the era of revolutions may close, and work become the sole object of all. The "spirit of the hive" is prudent and thrifty, but by no means parsimonious. And thus, aware, it would seem, that nature's laws are somewhat wild and extravagant in all that pertains to love, it tolerates, during summer days of abundance, the embarrassing presence in the hive of three or four hundred males, from whose ranks the queen about to be born shall select her lover; three or four hundred foolish, clumsy, useless, noisy creatures, who are pretentious, gluttonous, dirty, coarse, totally and scandalously idle, insatiable, and enormous.

But after the queen's impregnation, when flowers begin to close sooner and open later, the spirit one morning will coldly decree the simultaneous and general massacre of every male. It regulates the workers' labours with due regard to their age; it allots their task to the nurses who tend the nymphs and the larvæ, the ladies of honour who wait on the queen and never allow her out of their sight; the house-bees who air, refresh, or heat the hive by fanning their wings, and hasten the evaporation of the honey that may be too highly charged with water; the architects, masons, wax-workers, and sculptors who form the chain and construct the combs; the foragers who sally forth to the flowers in search of the nectar that turns into honey, of the pollen that feeds the nymphs and the larvæ, the propolis that welds and strengthens the buildings of the city, or the water and salt required by the youth of the nation. Its orders have gone to the chemists who ensure the preservation of the honey by letting a drop of formic acid fall in from the end of their sting;

to the capsule makers who seal down the cells when the treasure is ripe, to the sweepers who maintain public places and streets most irreproachably clean, to the bearers whose duty it is to remove the corpses; and to the amazons of the guard who keep watch on the threshold by night and by day, question comers and goers, recognize the novices who return from their very first flight, scare away vagabonds, marauders and loiterers, expel all intruders, attack redoubtable foes in a body, and, if need be, barricade the entrance.

Finally, it is the spirit of the hive that fixes the hour of the great annual sacrifice to the genius of the race: the hour, that is, of the swarm; when we find a whole people, who have attained the topmost pinnacle of prosperity and power, suddenly abandoning to the generation to come their wealth and their palaces, their homes and the fruits of their labour; themselves content to encounter the hardships and perils of a new and distant country. This act, be it conscious or not, undoubtedly passes the limits of human morality. Its result will sometimes be ruin, but poverty always; and the thrice-happy city is scattered abroad in obedience to a law superior to its own happiness. Where has this law been decreed which, as we soon shall find, is by no means as blind and inevitable as one might believe? Where, in what assembly, what council, what intellectual and moral sphere, does this spirit reside to whom all must submit, itself being vassal to an heroic duty, to an intelligence whose eyes are persistently fixed on the future?

It comes to pass with the bees as with most of the things in this world; we remark some few of their habits; we say they do this, they work in such and such fashion, their queens are born thus, their workers are virgin, they swarm at a certain time. And then we imagine we know them, and ask nothing more. We watch them hasten from flower to flower, we see the constant agitation within the hive; their life seems very simple to us, and bounded, like every life, by the instinctive cares of reproduction and nourishment. But let the eye draw near, and endeavour to see; and at once the least phenomenon of all becomes overpoweringly complex; we are confronted by the enigma

of intellect, of destiny, will, aim, means, causes; the incomprehensible organization of the most insignificant act of life.[4]

Other authors, like Driesch, give the postulated controlling agency the sharper outlines of a would-be scientific but in reality metaphysical entity and call it the "entelechy." It is true that the entelechy is deduced by Driesch from the autonomic peculiarities of the personal organism, but as the colony has all the essential attributes of the organism, he would undoubtedly assign it an entelechy, which according to the definition would have to be nonspacial, but working into space, nonpsychic, but conceivable only after analogy with the psychic, and non-energetic, but nevertheless capable of determining the specificity of the colonial activities through releasing and distributing energy.

I confess that I find the entelechy quite as useless an aid in unraveling the complex activities of the ant-colony as others have found it in analyzing the personal organism. This angel-child, entelechy, comes, to be sure, of most distinguished antecedents, having been mothered by the Platonic idea, fathered by the Kantian *Ding-an-sich*, suckled at the breast of the scholastic *forma substantialis* and christened, from a strong family likeness, after old Aristotle's darling ἐντελέχεια, but nevertheless, I believe that we ought not to let it play about in our laboratories, not because it would occupy any space or interfere with our apparatus, but because it might distract us from the serious work in hand. I am quite willing to see it spanked and sent back to the metaphysical household.

But, speaking seriously, it seems to me that if the organ-

[4] *The Life of the Bee*, tr. by Alfred Sutro (New York: Dodd, Mead & Co., 1901).

ism be inexplicable on purely biological grounds, we should do better to resort to psychological agencies like consciousness and the will. These have at least the value which attaches to the most immediate experience. And even the subconscious and the superconscious are more serviceable as explanations than such anaemic metaphysical abstractions as the entelechy. Of course, psychic vitalism is one of Driesch's pet aversions and he will have none of it, because he is a solipsist, but the fact that he is compelled to operate with a "psychoid" and with an entelechy conceivable only *per analogiam* with the psychic, shows the inconsistency of his position.

Before we can adopt any ultrabiological agencies, however, except in a tentative and provisional manner, an old and very knotty problem will have to be more thoroughly elucidated. I refer to the problem of the correlation and cooperation of parts. If the cell is a colony of lower physiological units, or biophores, as some cytologists believe, we must face the fact that all organisms are colonial or social and that one of the fundamental tendencies of life is sociogenic. Every organism manifests a strong predilection for seeking out other organisms and either assimilating them or coöperating with them to form a more comprehensive and efficient individual. Whether, with the mechanists, we attribute this tendency to chemotropism or cytotropism, or with the psychic neovitalists interpret it as conscious and voluntary, we certainly cannot afford to ignore the facts. The study of the ontogeny of the person, that is, the person in the process of making, in the hands of recent experimentalists, has thrown a flood of light on the peculiarities of organization, but the animal and plant colony are in certain respects

more accessible to observation and experiment, because the component individuals bear such loose spacial relations to one another. Then too, the much simpler and more primitive organismal type of the colony, as compared with that of the person, should enable us to follow the process of consociation and the resulting physiological division of labor more successfully. In the problem, as thus conceived, we must include, not only the true colony and society, and the innumerable cases of symbiosis, parasitism, and cœnobiosis, but also the consociation and mutual modification of hereditary tendencies in parthenogenetic and biparental plants and animals, since in all of these phenomena our attention is arrested not so much by the struggle for existence, which used to be painted in such lurid colors, as by the ability of the organism to temporize and compromise with other organisms, to inhibit certain activities of the aequipotential unit in the interests of the unit itself and of other organisms; in a word, to secure survival through a kind of egoistic altruism.[5]

[5] Since this paragraph was written I have found that several recent authors have given more explicit expression to a very similar conception of the role of coöperation and struggle in the development of organisms. Especially worthy of mention in this connection are Kammerer ("Allgemeine Symbiose und Kampf ums Dasein als gleichberechtigte Triebkräfte der Evolution," *Archiv für Rassen- und Gesellschafts-Biologie*, VI, 585–608, Leipzig, 1909); Schiefferdecker ("Symbiose," *Sitzungsberichte der Niederrheinischen Gesellschaft für Natur- und Heilkunde zu Bonn*, XIII, June 1904); Bölsche ("Daseinskampf und gegenseitige Hilfe in der Entwicklung," *Kosmos*, VI, Stuttgart, 1909); and Kropotkin (*Mutual Aid a Factor of Evolution*, New York: McClure, Phillips & Co., 1902).

II

JEAN-HENRI FABRE [1]

THROUGH the death of Jean-Henri Fabre on October 11, 1915, the world lost its greatest entomologist, a man who combined in an extraordinary degree the gifts of a virile and penetrating observer and those of a literary artist of high distinction. During the greater portion of a life of poverty, extending over a period of ninety-two years, he ceaselessly devoted himself to an intensive study of insect behavior and to the recording of his observations in such fascinating language that Victor Hugo styled him the "Homer of the insects."

Like the life of his countryman Latreille, who preceded him as the "prince of entomologists," Fabre's life was uneventful. His biography has been written by a sympathetic admirer, C. V. Legros, and rendered into English by another admirer, Bernard Miall, but from many passages scattered through Fabre's great work, the *Souvenirs Entomologiques*, it is possible to glean an even more illuminating and intimate knowledge of his powerful individuality and of his methods of working and thinking. He was born of humble peasant parents on December 22, 1823, in the hamlet of Saint Léon, in the part of Provence known as the Haute-Rouergue. Through diligent application to the classics, physics, chemistry, and mathematics in the rather mediaeval

[1] From *The Journal of Animal Behavior*, vol. VI (New York, 1916); included here by permission of The Williams & Wilkins Company.

schools of his day he prepared himself to become a teacher. At nineteen he entered on this profession in the College of Carpentras and in 1850 accepted a position as professor in the *lycée* of Ajaccio, Corsica, at a salary of £72. Here he met the naturalist Moquin-Tandon, who seems to have had an important influence in determining his career as a biological investigator. Even as a boy, however, Fabre had been greatly interested in insects, so that Moquin-Tandon probably only helped to reveal to him his innate aptitude for observation and experimentation. He realized that he had a genius for observing small animals, and from that time forth, like Socrates, he implicitly obeyed the voice of his daemon almost to the hour of his death. Falling ill with malaria at Ajaccio he was compelled to return to France, and in 1853 was appointed assistant professor of physics at the *lycée* of Avignon. This post he held for nearly twenty years (till 1871), without advancement and with a salary not exceeding £64! During this period he made some of his most important observations. The written accounts of his work, contributed to the *Annales des Sciences Naturelles*, include a study of the habits of the solitary wasp *Cerceris* and of the cause of the long conservation of the beetles on which it feeds (1855), notes on the life-history of *Cerceris*, *Bembex*, and *Sitaris* (1856), followed by his classic memoir on the hypermetamorphosis and habits of *Sitaris* (1858) and studies on the role of adipose tissue in the urinary secretion of insects (1862).

In 1871 he left the *lycée* of Avignon to devote the remainder of his life to the study of instinct in insects. He moved to Sérignan, a hamlet near Orange, not far from Avignon. Here he lived henceforth and worked as a hermit

and here he died, in a little cottage on a plot of ground called the "harmas," a beautiful description of which is given in the opening chapter of the second series of *Souvenirs*.[2] During the early years of his residence at Sérignan he was compelled to devote much time to writing textbooks on natural-history subjects for the purpose of keeping the wolf from the door. It will probably be found that these little books were the forerunners of the modern "nature books." He also continued to contribute scientific articles to the *Annales des Sciences*. Two of these on the habits and parthenogenesis of some bees of the genus *Halictus* (1880) and one on the repartition of the sexes in the Hymenoptera (1884) are of unusual interest. But the great work accomplished at Sérignan is embodied in the ten volumes of the *Souvenirs*. Anything like an adequate review of this monumental work would require much time and labor. Only the entomologist who has endeavored to work out complicated insect life-histories will fully appreciate Fabre's powers as an observer and will not be greatly surprised to learn that during the course of years he wore a groove in the stone floor of his laboratory by walking around his table. The complete elucidation of some of the life-histories, like that of the sacred scarabaeus, required observations extending over a period of nearly forty years.

The newspapers and magazines have made us familiar with the romance of the closing years of Fabre's life. Mistral, the Provençal poet, Maeterlinck, and Rostand are said to have discovered Fabre and to have called the attention of the world to his destitute condition. According to a well-

[2] Translated by A. T. de Mattos in *The Life of the Fly* (New York: Dodd, Mead & Co., 1913).

known French magazine, "In 1910 he was revealed to the people; a group of litterateurs and savants conceived the idea of offering this modest, almost unknown man a plaque to perpetuate the memory of his work. Two years later his ninetieth birthday was celebrated by a ceremony at which the Institut was represented, and somewhat later the President of the Republic paid him a visit." "Why," asks Legros at the close of his account of the celebration of 1910, "at this jubilee of the greatest of entomologists, was not a single appointed representative of entomology present?" And he goes on to say: "The fact is that the majority of those who 'amid the living seek only for corpses,' according to the expression of Bacon, unwilling to see in Fabre anything more than an imaginative writer, and being themselves incapable of understanding the beautiful and of distinguishing it in the true, reproached him, perhaps with more jealousy than conviction, with having introduced literature into the domains of science." This is an unfair statement of the case. Fabre has long been known to naturalists and especially to entomologists, and many of them, from Darwin to the Peckhams and Forel, have referred to his work in terms of the greatest admiration. It is only the literati and general public who have just discovered Fabre, and it is not difficult to account for this belated appreciation. Insects are so peculiarly organized and offer to the casual observer so few points of contact with the general trend of human interests that even the magic style of a Fabre failed to elicit a widespread desire to know about their activities. But when a great writer like Maeterlinck announced that, "Henri Fabre is one of the greatest and purest glories in the present possession of the civilized world, one of the most erudite

naturalists and the most marvellous poet in a modern and truly legitimate sense of the word," and added that Fabre was one of the profoundest admirations of his life, people who had never devoted five minutes of their lives to studying an insect began to sit up and take notice.

Whenever there is complaint of the neglect of a genius by the world, it is well to scrutinize the behavior of the genius. If we do this in Fabre's case we shall have little difficulty in accounting for the neglect from which he suffered, both on the part of the general public and the scientific fraternity. Fabre and his publisher are undoubtedly responsible for much of the popular neglect. The title of the great work, *Souvenirs Entomologiques*, is inept, to say the least; the ten volumes were unattractively printed and inadequately illustrated, and the arrangement of the articles in the series might have been much better. All rights of translation, even of extracts, were, moreover, rigidly withheld till very recently. When Fabre became more generally known these faults were corrected by the publication of selected essays in more pleasing volumes and under more appropriate titles, such as *La Vie des Insectes* and *Mœurs des Insectes*, an arrangement which has been followed in the English translations now displayed in all our bookstores.

The reserved and unsympathetic attitude of entomologists towards Fabre was very largely due to the fact that he was a crotchety and opinionated recluse, who seems never to have made the slightest attempt to enter into friendly personal or epistolary relations with other entomologists, who never mentioned and probably never read the work of his contemporaries, who lost no opportunity of holding up to ridicule some of the most important entomological studies,

such as insect taxonomy, and who repeated investigations that had been made by others, without intimating and evidently without knowing that such investigations had long been known to the entomological world. He failed to realize that entomologists are more human than the objects of their studies and that *wer Liebe ernten will, muss Liebe säen*. It is not surprising, therefore that there was no afflux of entomologists to Sérignan to celebrate his jubilee. While their presence might have been commendable, we can hardly blame them, under the circumstances, for staying at home.

Another reason for the attitude of entomologists towards Fabre is to be found in his peculiar views concerning instinct, views that were prevalent enough in the early part of the nineteenth century but are singularly foreign to the psychology and theoretical biology of the present day. He not only declined to accept the doctrine of evolution but vigorously attacked it in more than one of his essays, although many of his criticisms so far overshoot the mark that one reads them with amazement. It must be remembered, of course, that Fabre was nearly thirty-six years old when the *Origin of Species* appeared. He was not, therefore, like the naturalists of the present generation, suckled, so to speak, at the breasts of evolutionary doctrine, and his life-long lack of contact with biological speculation kept him from viewing the phenomena of instinct from a genetic standpoint. But even in his chosen field, the study of instinct, he confined himself to a comparatively circumscribed group of phenomena. He worked only on a series of insects selected from his immediate environment and for certain peculiar reasons took little interest in the social species (ants, social bees and wasps), which are, nevertheless, abundant in south-

ern France. His studies on those forms are limited to an essay on the amazon ant (*Polyergus rufescens*) and a few essays on wasps (*Vespa vulgaris*). He seems to have borne a grudge against the ants because they so often entered his breeding cages and killed the insects with which he was experimenting. There were also other and more weighty reasons for this neglect of the very insects which naturally suggest a genetic interpretation of instinct. Fabre believed that instinct manifests itself in its purest form in the solitary species. He was, moreover, greatly impressed by its fixity and mechanical aspect, and his rigid training in physics, chemistry, and mathematics and his keen analytical ability probably biased him in favor of views which have grown more and more repugnant to modern biologists. He had a strong tendency to schematize his observations and to ignore the variability of instinct. This tendency has been pointed out by several observers and is most clearly marked in his classic work on the solitary wasps.

Although Fabre suffered in the estimation of many biologists on account of the theoretical views which he elaborated and very stubbornly upheld through the course of a long life in voluntary isolation from the great current of biological thought, we are bound to confess that in some very important matters his vision was clearer than that of his contemporaries. He alone realized the great significance of the study of animal behavior at a time when other biologists were absorbed in purely morphological work. No better proof of this statement can be given than the concluding paragraphs of the first essay in the second series of the *Souvenirs* published in 1882. I quote from De Mattos' translation (pages 26 and 27):

Laboratories are being founded, at great expense, on our Atlantic and Mediterranean coasts, where people cut up small sea-animals, of but meagre interest to us; they spend a fortune on powerful microscopes, delicate dissecting instruments, engines of capture, boats, fishing crews, aquariums, to find out how the yolk of an Annelid's egg is constructed, a question whereof I have never yet been able to grasp the full importance; and they scorn the little land-animal, which lives in constant touch with us, which provides universal psychology with documents of inestimable value, which too often threatens the public wealth by destroying our crops. When shall we have an entomological laboratory for the study not of the dead insect, steeped in alcohol, but of the living insect; a laboratory having for its object the instinct, the habits, the manner of living, the work, the struggles, the propagation of that little world, with which agriculture and philosophy have most seriously to reckon?

To know thoroughly the history of the destroyer of our vines might perhaps be more important than to know how this or that nerve-fiber of a Cirriped ends; to establish by experiment the line of demarcation between intellect and instinct; to prove, by comparing facts in the zoological progression, whether human reason be an irreducible faculty or not; all this ought surely to take precedence of the number of joints in a Crustacean's antenna. These enormous questions would need an army of workers, and we have not one. The fashion is all for the Mollusc and the Zoophytes. The depths of the sea are explored with many drag-nets; the soil which we tread is consistently disregarded. While waiting for the fashion to change, I open my harmas laboratory of living entomology; and this laboratory shall not cost the ratepayers a farthing.

Not only was Fabre the first to realize the full importance of a scientific study of animal behavior, but he was the first consistently to apply the experimental method to the investigation of the animal mind. The *Souvenirs* abound in accounts of experiments, performed for the purpose of eluci-

dating the nature of instinct, not the less illuminating and conclusive because they were carried out with crude, home-made apparatus. It is as instructive as it is humiliating to read his results and to reflect on the mountains of complicated apparatus in our modern laboratories and the ridiculous mice in the form of results which only too frequently issue from the travail of "research."

Another valuable service of Fabre consisted in his calling attention to the fact that the applications of zoology to human welfare must be based on an accurate knowledge of animal behavior. This has been tacitly assumed by economic entomologists, but neither they nor the modern behaviorists have sufficiently emphasized the fact that we cannot hope to control animal depredations or to compel animals to contribute to our well-being and wealth without an exhaustive knowledge of such apparently remote phenomena as the animal sensations, reactions, and instincts. Fabre is quite explicit in this matter. Though he lived as a recluse from the scientific world, he remained in intimate contact with the life of the Provençal peasant and had no doubts concerning the important bearing of his own work on such a fundamental industry as agriculture.

III

ON INSTINCTS [1]

[*Preliminary Note*: — Professor Hocking, who discussed the instincts before I became a member of the club, has very generously permitted me to read a manuscript of some chapters containing, I presume, a résumé of his discussion. The manuscript did not reach me till my own paper was nearly completed, or I should have made several changes, mainly in the direction of rounding off certain asperities and mitigating certain emphases. We have both been viewing the same large and complicated subject, but as might be expected, from different angles, Professor Hocking, as a philosopher and psychologist mainly from the human side and more introspectively and I, as a zoologist, from the animal side and more concretely and objectively. Professor Hocking is, I suppose, what Jung would call an introvert, whereas I am an extrovert. His picture of the instincts has classic proportions and is painted with delicate, deft brush-strokes in subdued colors, whereas mine can only be called impressionistic, or, perhaps, cubist. It is done with a broom dipped in whitewash, tar, and other crude pigments and you will probably agree that it looks best from a distance of at least a mile. I have limited my treatment mainly to a brief history of the *Instinktbegriff*, a sketch which on reperusal impressed me as being very inadequate, and a consideration of the methods of studying instincts, which is too dogmatic and combative. After I had reached this estimate of my paper, too little time was left in which to eliminate its many defects, to say nothing of writing a new paper, so that I shall have to read what I have written, even if it wrecks the club.]

THE ECONOMIC entomologist is primarily engaged in a study of the relations of insects to one another, to other organisms, and especially to man. All such relations, in so far as they involve the human race, may be distinguished as either actually or potentially beneficial or injurious, and are, of course, due to peculiarities of behavior.

[1] Read before the Royce Club, May 20, 1917, and published in the *Journal of Abnormal Psychology* (Boston), December 1920–March 1921; included here by permission of the American Psychological Association.

And since behavior, both in insects and in man, is fundamentally of the type called instinctive, the economic entomologist, far from having to apologize for an interest in the perennial problem of instinct, would be deserving of censure if he failed to keep it constantly in mind. Moreover, no class of organisms offers such a marvellous field for the study of instincts as the Insecta. No other class, with the possible exception of birds, shows anything like the diversity and complexity of these phenomena and, owing to the great number of species, genera, and families that have survived the vicissitudes of geologic time — a number far in excess of that of all other organisms on the planet — no other class exhibits such a complete representation of the historical or phylogenetic stages of many instincts.

As mere phenomena the instincts are well known, and there is practical unanimity among authors in regard to their peculiarities. Any behavior is designated as instinctive which originates in an impulse, but the nature of impulse cannot be defined further than to say that it has both a conative and a cognitive aspect. Those who lay greater emphasis on the conative aspect prefer to use such terms as impulse, *Trieb*, *horme*, life urge, *élan vital*, etc., whereas those who wish to suggest the cognitive aspect use such terms as craving, appetite, desire, interest, libido, etc. The impulse is evidently the center or core of the instinctive activity, which is peculiarly fixed and mechanized, very rigidly dependent on inherited structure or organization and therefore very uniform, or variable only within rather narrow limits, in all the individuals of one or both sexes of a species. Behavior of this kind has the attributes of compulsion or necessity and is at the same time highly adaptive or pur-

posive, though the organism manifesting it is unaware of any purpose, or at any rate is usually aware only of an immediate purpose, even when the behavior is accompanied by consciousness. This is the classic description of instinct as it recurs, with unimportant modifications, in countless works on the subject. It is perfectly evident that the description is essentially that of the life process itself and that we are unable, except by resorting to the artificial method of conceptualization, to confine instinct within definite descriptive limits.

When we endeavor to get at the meaning of the phenomena called instinctive we enter, as Wundt has remarked, a veritable museum of opinions. As few attempts have been made to write a history of the *Instinktbegriff* from pre-Aristotelian times to the present, you will pardon me for making a feeble attempt at a hasty sketch of such a history, especially as my point of views differs from that of the two Neo-Darwinians, Gross and Ziegler, the only authors who seem to have discussed the matter. We can recognize in early Greek thought the common undifferentiated source of three very vital currents of opinion on the subject of instinct which have flowed down to our own time, sometimes apparently blending, but always again separating and now occupying well-worn channels though occasionally exhibiting a tendency to branch and form new streams. These three currents of opinion, which correspond also to three different ways of looking at the problem of life in general, I shall designate as the theological or teleological, the physiological or mechanistic, and the psychological or anthropomorphic.

The Oriental and early Greek philosophers, in accordance

with their naive and natural attitude towards the animate world, made no sharp distinctions between the human and animal soul. There was therefore nothing to prevent animals from becoming the temporary abodes of human souls as assumed in the Hindoo, Pythagorean, and Plutarchian belief in metempsychosis. That the common people among the ancients held similar views is clearly indicated by such compendia of folklore as Ovid's Metamorphoses and the Golden Ass of Apuleius. Such views are, indeed, the natural product of a people in the hunting and pastoral stages of culture. Moreover no one will deny that even the sexual relations of men, gods and animals are very much mixed in ancient folklore.

The more specific theological view of instinct seems to me to have had its origin in the use of animals for purposes of augury or divination, although no one, to my knowledge, has advanced this opinion. Primitive man and even the peasantry of highly civilized peoples are always deeply impressed by the fact that animals adapt their behavior to the meteorological conditions and attribute this adaptation to prevision or prescience. What is more natural, therefore, than to believe that such foresight is derived directly from the Deity and that it can be exploited for the benefit of mankind? The belief in the divine origin of instinct is expressed in many ancient writings but nowhere more beautifully than in Virgil's lines on the bees in the fourth book of the Georgics:

> These acts and powers observing, some declare
> That bees have portion in the mind of God
> And life from heaven derive, that God pervades
> All lands, the oceans, plains, the abyss of heaven,

> And that from him flocks, cattle, princely men,
> All breeds of creatures wild, receive at birth
> Each his frail, vital breath; that whence they came
> All turn again, dissolving.[2]

The path of development of the theological view of instinct can be clearly traced through Plato, Aristotle, the Stoics, and the Scholastics into modern theology. In Plato's earlier writings metempsychosis still has a place, but later, in his emphasis on the divine nature of the λογιστική or reason and its primacy in the human soul, he created a rift between man and the animals which was destined in the course of time to widen into a chasm. Aristotle gave greater precision to Plato's view with his distinction of the *anima intellectualis* and *anima sensitiva*, the former being peculiar to man, the latter common to man and animals. But Aristotle still left room for some belief in a gradual development of the animal into the human soul. The Stoics elaborated the Aristotelian conception and seem to have been among the first to use the word ὁρμή for what the Latins called *instinctus* (from *instinguere*, to incite). The Stoics conceived the ὁρμή to have been implanted in animals by God and, as we learn from the opinions of Crysippus reviewed by Cicero in the second book of the *De natura deorum*, they laid great stress on its teleological aspect and were as expert in the use of the argument from design as any parson of early Victorian England. The Christian theologians failed to find any better interpretation of instinct and merely gave the Stoic and Aristotelian views a formulation which in their

[2] T. C. Williams' translation, *The Georgics and Eclogues of Virgil* (Harvard University Press, 1915).

opinion settled the problem of the animal soul for all time. In the thirteenth century, St. Thomas Aquinas converted the distinction between the human and animal soul into an impassable gulf by his contention that when God created the world he implanted instinct in the animals and the free intelligence of the angels in man. As the angelic doctor and his successors could not deny to animals memory and the ability to profit by their individual experience, these endowments were added to instinct and intelligence was treated as synonymous with ratiocination. All traces of the genetic germ in Aristotle's conception had evaporated, and the teleological aspect of instinct was conceptualized and exploited with all the astuteness of which scholasticism was capable. As a consequence, the animal world was treated as psychically uniform, and there was no encouragement to inquire into such differences of behavior as separate the polyp from the elephant or the earthworm from the chimpanzee. The whole scheme, if not actually devised for the purpose of making the human soul more amenable to ecclesiastical control, has, at any rate, most effectually accomplished that result. Ever since the days of St. Thomas the theological view of instinct and intelligence has been repeated by Catholic and Protestant divines alike with a monotonous reiteration which affords one of the most striking illustrations of the limitations of the clerical mind, and from Cicero, that William Jennings Bryan of antiquity, down to Reimarus, Paley, and Kirby, the author of the Seventh Bridgewater treatise, all the facts accumulated by the observers of animals are manipulated according to the same old formula and converted into ecclesiastical propaganda.

Perhaps it is not generally known that of late there has

been a great revival of Thomistic teaching in the Jesuit schools as a result of one of the encyclicals of Pope Leo XIII. That this revival has given the decrepit theological theory of instinct a new lease of life is apparent from the works of the Rev. Erich Wasmann S. J., an investigator justly admired for his remarkable studies on ants and termites and their guests and parasites. One of his works on the interesting beetle *Rhynchites betulae*, the "Trichterwickler" of German entomologists, is a fine example of the theological method of studying instinct. The *Rhynchites* makes two transverse incisions in a birch-leaf and folds up the apical portion in the form of a compact case for its eggs and larvæ. The lines of incision have been shown to be mathematical curves of such a nature as to represent the evolute of an evolvend and therefore to produce the leaf-area precisely suited to the beetle's purpose.

Wasmann concludes that the *Rhynchites* has been familiar with the solution of the problem of constructing the evolute of a given evolvend since the time of Adam, although Adam's descendants did not succeed in finding the solution till 1673, when Huyghens applied the differential calculus to geometry. Wasmann closes his study of the behavior of the beetle with the remark that "the *vis aestimativa* of St. Thomas Aquinas is even today and in the light of modern research, the best explanation of instinct," and with a little hymn in which the *Rhynchites* is exhorted to praise his Creator:

Kleiner Trichterwickler, preise
Deines Schöpferarmes Macht
Preise Seiner Weisheit Wege
Die Kein Erdengeist erdacht;

Preise Seine Vaterliebe
Deren Treue ewig wacht;
Preise sie nach alter Weise
In der glaubenslosen Nacht.

We can only congratulate the Trichterwickler on the effortless acquisition of his mathematical knowledge and suggest that he repeat as a refrain the famous stanza from the witch's song in Faust:

Die hohe Kraft der Wissenschaft
Der ganzen Welt verborgen,
Und wer nicht denkt, dem wird
sie geschenkt,
Er hat sie ohne Sorgen.

And after commiserating Huyghens and ourselves on not having had the good fortune to be born Trichterwicklers, let us return to our historical sketch.

I may include the metaphysicians with the theologians because they have handled instinct in essentially the same fashion. When they disengaged themselves from the influence of the theologians and began to create their Weltanschauungen they merely made instinct a manifestation of their own peculiar gods. Thus Schelling attributed instinct to the "allwohnende Vernunft," G. F. Schubert to the "Erdpsyche," Schopenhauer to the "Wille als Weltprinzip," von Hartmann to the "Unconscious," Nietzsche to the "Wille zur Macht," Driesch to the "entelechy," Bergson to the "élan vital," and Newland to "telesthæsia" and "telepathy." As the type of mind which delights in such constructions is, like the clerical mind, if we may judge from history, an ever recurring sport or mutant among human generations, comparable to DeVries' *Oenothera lata* or *oblonga* among

the evening primroses or albinos among rodents, the future undoubtedly has in store for us many more explanations of like import.

The mechanistic or physiological view of instinct seems to be as ancient as the theological, and exhibits a similar vitality. It is the expression of another kind of sport, or mutant, of which the earliest known examples are Democritus, Epicurus, and Lucretius. These thinkers have always suffered harsh treatment at the hands of the theologians, and their ilk were either gagged or exterminated during the middle ages. In recent times, however, they have more than made up for centuries of enforced silence by becoming so vociferous and dogmatic that nothing else can be heard in the biological camp. When they turned their attention to the instincts they ousted the concept of teleology and attacked the workable mechanical manifestations with zeal and truly marvelous success. They accepted Descartes's interpretation of animals as creaking machines, but threw his Jesuitical philosophy overboard and ended by becoming shockingly materialistic. They began to write the term instinct with a small letter and preferred its plural form. More recently they have taken to writing it in quotation marks or drop it altogether and resort to circumlocution when they discuss animal behavior. They regard instinct as nothing but a compound or catenary reflex determined by inherited structure of the nervous system. At bottom their view is no more evolutionary or genetic than that of the theologians, but when organic evolution was promulgated in Darwinian guise the mechanists embraced it with effusion and had no difficulty in showing, at least to their own satisfaction, how the instincts must have developed

during phylogeny. They made the important discovery that the instincts of the same species often vary appreciably and concluded that this variation and that of the associated structures was indefinite. Natural selection and the survival of the fittest could then be introduced and the problem was solved. Of course, grave difficulties were encountered in the genesis of intelligence and consciousness, but these could either be ignored, minimized, slurred over, or produced by a kind of prestidigitation from conflicting unconscious instincts or reflexes. The argument for natural selection was greatly assisted by fishing in the troubled waters of certain extraordinary entomological and botanical phenomena, which had not even been adequately observed and described as phenomena, such as protective resemblance and mimicry, the sterility of worker castes among the social insects, the relations of floral structures to insects, the supposedly passive adaptations of plants, such as those of wind-borne seeds and brilliantly colored fruits, and especially the instincts which are manifested only once and with great adaptive perfection during the life of the individual organism. I shall have another occasion to refer to these instincts, for which I would coin the term *hapaxoræic* [*from* ἅπαξ, once, and ὡραῖος, ripening]. The mechanistic or physiological view of instinct may be said to have reached its fullest expression in the works of Weismann, E. Ray Lankester, Poulton, Loeb, Bethe and many others, and most of our speculative biological and not a little of the current sociological and economic literature is now saturated with the assertions of these investigators and their disciples.

The psychological or anthropomorphic view of instinct is probably as old as the other views I have been considering.

It can be traced as far back as Heraclitus, Pythagoras, and Empedocles but more clearly in the writings of Plutarch and the Neo-Platonist Porphyrius. In the eighteenth century it reappears in the works of Montaigne, Condillac, and Leroy. It is essentially a recognition of the fundamental identity of the animal and human souls, the differences between which are regarded merely as differences of degree of development. This implies that intelligence is not exclusively human and that the animal mind is to be interpreted in the light of human psychology, a procedure which is, of course, open to the charge of anthropomorphism. But owing to the fact that all our science is necessarily anthropomorphic and that man himself is merely a highly developed animal and therefore could not if he would interpret other animals except in terms of himself, the charge can only mean that he continually runs the risk of attributing to animals a greater development of intelligence than they possess. As scientific methods of observation and experiment are quite adequate for the control and rectification of this tendency, it is impossible to regard anthropomorphism as such a very terrible eighth mortal sin. It is strange, nevertheless, that only one modern biologist, the late A. D. Darbishire, has had the courage to look that opprobrious and sonorous epithet squarely in the eye. He says:

> The great length of the word, and its constant repetition, may in some degree account for its impressive effect and for its anaesthetic influence on the critical faculty. But be this as it may — and I intend it as no more than a tentative suggestion — there can be no doubt that the word anthropomorphism affords a very good instance of the baneful effect which a word may have on the course of thought. In its original restricted signification, in which it meant the endowing of God with the form and habits

of man, it certainly denoted a grave intellectual misdemeanor, and the epithet anthropomorphic, which very accurately described this process, was rightly regarded as a stigma. But those who were responsible for the extension of the meaning of the word at the "endowed" end, for applying the word anthropomorphic to an entirely different thing — the granting of intelligence, purpose, design and human attributes in general to non-human animals, in order to stigmatise a concession to the "lower animals" which was repugnant to them — were the unconscious perpetrators of a successful fraud. One of the easiest ways to convince an audience of the untruth of an idea you wish to disprove is to apply to that belief a word which has already been brought into discredit and obloquy. If you can persuade the audience that the word fits, the trick is done. In the case of the word anthropomorphism the audience needed no persuasion; they hated the idea that an animal had a soul, many of them hated the idea that they themselves had a soul; they liked to think of the organism as a machine, they liked their mechanical theory of evolution and they liked a long word. The belief that a non-human animal has an intelligence at all comparable to their own was branded with the word anthropomorphic, and flung into the ash-bin of exploded superstitions. It was an argument which effected the temporary expulsion of this belief; it was abuse. It was the very essence of abuse — which is calling things names.

The psychological view of instinct has certain great advantages. It is naturally genetic and favorable to the interpretation of organisms as historical beings. Instinct and intelligence are not regarded as separate faculties but as extreme phases of one psychical process which in our individual experience is continually lapsing from conscious and intelligent performance to the mechanized status of habit. The same process is supposed to have gone on throughout the phylogenetic history of the organic world and to have resulted in all the characteristic structures and behavior of

existing organisms. In other words, instinct is essentially inherited habit. Hence individual experience, which is rejected as of no value by the Neo-Darwinians in comparison with the fortuitous concourse of accidental germplasma variations, must affect, at least in some measure, the constitution of succeeding generations. Expressing such a view means of course committing the ninth mortal sin, known as Lamarckism, which is faith in the inheritance of acquired characters and in the opinion that the function creates the organ. I can only remain impenitent and state my conviction that structure is after all the visible, highly mechanized end-stage of function and that our inability to detect the inheritance of an acquired character is probably due to the fact that its visible appearance is preceded in phylogeny by a period of many generations during which it is inherited only as a function associated with alterations of structure too subtle to be revealed by our present very crude methods of observation and experiment. Inheritance of alternative characters in definite Mendelian ratios would therefore be merely the method of inheritance of the stereotyped end-products of a long evolution and would not represent the actual phylogenetic method of the development of such characters.

The highly adaptive and teleological aspect of the instincts also becomes clear on the foregoing suppositions, since the completed instinct is merely the congealed result, so to speak, of more fluid or unstable activities of the random, trial and error, or perseverance type initiated and guided by a feeble intelligence. The argument used by my old teacher, Prof. C. O. Whitman, and repeated by Holmes, that lapsed intelligence cannot account for instinct, because the phylogenetic

sequence in the animal kingdom is instinct in the lower, followed by intelligence in the higher forms, seems to me to be easily answered, if we admit what the researches of Jennings and others seem to compel us to admit, that even the lowest organisms have a glimmer of intelligence and that all organisms have a truly astonishing ability and tendency to form habits. If this is true a very feeble intelligence could conceivably build up in the course of ages a considerable and complicated fabric of instincts and structures, a fabric so impressive that in all plants and in many animals we might be unable to detect the diminutive intelligence by which it had been so slowly and painfully initiated and elaborated.

A view of the instincts essentially like the one I have expounded, but expressed in somewhat different and possibly more intelligible language, has of course been held by a long line of eminent zoologists and psychologists, including Lamarck, Darwin, Romanes, Samuel Butler, Cope, Hyatt, G. H. Lewis, Bain, Spencer, Eimer, Preyer, Wilser, Wundt, Ribot, and G. H. Schneider. Recently Paully, Francé, and Camillo Schneider have given this view a more neovitalistic formulation, while others, like Rignano, George Darwin, Semon, James Ward, Piéron, Brun, and Hartog have followed Hering and Samuel Butler in developing a mnemic school, the main contention of which is accepted by Haldane and expressed in the following luminous sentence: "In a living organism the past lives on in the present, and the stored adaptations of the race live on from generation to generation, waking up into response when the appropriate stimulus comes, just as conscious memory is awakened." [3]

[3] J. S. Haldane, *Organism and Environment* (New Haven: Yale University Press, 1917), p. 98.

What I wish to say concerning the methods of investigating instincts may be treated under three heads, the experimental, the historical, and the psychopathic. The theologians, metaphysicians, and sociologists have, of course, developed no peculiar methods of investigating biological phenomena. They take over, manipulate, and interpret the output of investigation and, to judge from the result, some of them ought to be forbidden by law to indulge in such practices. The experimental method, so universally applicable and successful in physics and chemistry, is certainly of much more limited service in the departments of biology that deal with the living organism. As would be expected the method is most successful when applied to those phenomena which are most thoroughly mechanized and tend to repeat themselves, that is, to the specialized and relatively stable end-stages of the life process. This is well seen in genetics, where very simple experimental methods, consisting of nothing but breeding plants and animals and sorting their offspring according to observable characters, have revealed the very startling uniformities of Mendelian inheritance. In the study of animal behavior important results have been achieved by experimentation in detecting the limits of the variations of instincts, in disposing forever of the notion of their infallibility, and in elucidating the relations between stimuli and responses. The serious limitations of the method lie in the fact that the living plant or animal is not a mere mechanical system but a creative organism, a being that cannot be isolated from its environment like a material system and one which has the ability to epitomize its whole past in its structure and behavior. It will always be necessary, therefore, to supplement experimentation with the historical

method. As this method, which has been so successfully employed by archaeologists and palaeontologists, has of late fallen into undeserved disuse and even disrepute among laboratory biologists, I wish to show how it can be applied in the interpretation of behavior by a somewhat detailed consideration of three typical and to the superficial observer very simple insect instincts, the spraying instinct of *Formica rufa*, the balloon-making instinct of the Empidid flies, and the spinning instinct of the caterpillars of moths and butterflies.

When the mound of any one of the numerous North American or Eurasian varieties of *Formica rufa* is disturbed, the workers at once rush to the most exposed surfaces of the nest, face the intruder, rise on their hind and middle legs, direct the tip of the abdomen forward, and shower him with a spray of formic acid so fine as to be invisible except in a favorable light. These ants have, therefore, long borne the old Shakespearian name of *pismire*, from the colloquial, onomatopoeic *piss* and *mire*, which is still the Dutch name for ant and has the same root as the Greek *myrmex*, the Latin *formica*, the Icelandic *maurr*, the Persian *mir*, etc. The Century and Webster's dictionaries are, of course, propagating an error when they state that the first syllable of the word pismire refers to "the strong urinous smell of an ant-hill." By bringing a bright metallic surface, such as the blade of a new pocket-knife, near the spraying ants it is found that they can project the acid several inches to a foot, as the surface instantly tarnishes or corrodes at such distances. The behavior of the ant has nothing to do with micturition, but is merely the discharging of a pungent, liquid, and volatile glandular secretion. Anyone who brings

his eyes too near a nest when the ants are working their apparatus under high pressure and the nest is enveloped in a cloud of acid fumes, will have no doubts about its value as a means of defense against the bears, skunks, rats, mice, and birds which often dig in the ant hills to get the larvæ and pupæ of which they are very fond. But the formic acid can also be used offensively and at very close range, as we see when we dump a lot of ants of an alien species on the nest. Then each *rufa* worker seizes an enemy with its mandibles, turns the tip of its abdomen forward to the wound made by these appendages and thoroughly drenches it with the acid. The secretion thus entering the blood of the victim causes instant paralysis or even death. When a *rufa* worker performs this operation on the human body the sensation is that of a sting, though it is not, as commonly supposed, produced by a sting. Shakespeare falls into this error when he makes Hotspur say in Henry IV (1.3, 240):

> Why, look you, I am whipp'd and scourg'd with rods,
> Nettled and stung by pismires.

When we examine the spraying outfit under the microscope we find it to consist of a voluminous, muscular sac or reservoir into which opens a tubular gland, several centimeters in length, but so slender and so densely coiled that it makes a small cushion applied to the inner dorsal wall of the sac. The latter opens at the tip of the abdomen with a very small circular orifice on a papilla fringed with a circlet of ciliary hairs. The organ is merely an atomizer in which the rubber bulb is represented by the muscular sac.

We now know the machine and how it works and my

friend Dr. Loeb would undoubtedly ask me: "What more do you want?" I would reply that I am one of those absurdly inquisitive people who might like to know how the pismire came into possession of its atomizer, whether it was a present, purchased, stolen, or made by the pismire itself. As I have no reason to suppose, however, that the pismires have passed in procession before the throne of the Almighty and have been handed their atomizers like so many diplomas, and as I have never heard of ants breaking into drugstores for the purpose of stealing atomizers or buying them over the counter and as I have actually seen stained sections showing various stages in the development and growth of the apparatus, I feel sure that each pismire must make its own atomizer, and my respect for the insect is greatly increased. But when I see how very quickly and deftly it makes the apparatus I suspect that after all it is so skillful merely because its living substance is really continuous in time with that of untold generations of pismires that have been in the atomizer business for millions of years. Of course, the only way I can satisfy my morbid curiosity in regard to the way this business was carried on in former ages is to pose as a benevolent old archaeologist and to ask all the pismires' sisters and cousins and aunts and all the remoter relations of the family Formicidæ to show me their atomizers or any similar heirlooms in their possession. A number have complied with my request and, to make a long story short, this is what I find:

Some of the most ancient and primitive insects, like their ancestors, the centipedes, had at the hind end of the body more legs than were needed for purely locomotor purposes. They therefore used them as implements for making holes

in the soft soil and took to laying their eggs in these holes. A more efficient implement was later perfected by bringing two or three pairs of the slender legs together to form a tube through the lumen of which the eggs could be passed into the soil by an up and down movement of the appendages on each other. The friction of these hard parts was then overcome by the use of a lubricating solution derived from one or more cutaneous glands that originally opened on the surface of the body at the bases of the appendages. Thus what is called an ovipositor with its lubricating gland was developed much as you see it in any female cricket you may chance to meet on a country roadside in October. In other insects, like the gall-flies and ichneumons, the ovipositor became shorter, stiffer, and more pointed at the tip, so that the eggs could be inserted in the hard woody tissues of plants and through the tough integuments of other insects. After acquiring this kind of an ovipositor the insect found that it could be used also as a weapon, and after the lubricating liquid had become poisonous it became a formidable sting. When this stage was reached two paths of development were opened up, one of which is seen in the solitary wasps, the other in the social wasps, social bees, and lower ants. In both groups the function of oviposition became dissociated from that of the sting, as the solitary wasps found it advantageous to sting their prey and thus kill or paralyze it and then simply to lay their eggs on its surface, so that the hatching larvæ could become external feeders, while the social wasps, social bees, and lower ants no longer needed an ovipositor because they had come to live in populous colonies and could easily defend their progeny with their stings. But the sting in the lower ants had still the disadvantage that it could be used only at

close range and therefore often imperiled the life of its possessor. This disadvantage was overcome by greatly increasing the size of the poison gland, the abundance of its secretion, and the contractility of its reservoir, so that the poison could be thrown to a distance in the form of a spray. At the same time for hand to hand encounters the mandibles could be used with telling effect and supplemented by the poison gland. That this has been the evolution of the pismire's atomizer is shown by the presence of a very minute and now functionless sting just within the anal orifice. Thus the insects have passed through various stages in the development of offensive and defensive organs just as man has passed from the stage, indicated by the Latin *comminus*, when he used his nails, teeth, fists, and the sword to the *eminus* stage, beginning among primitive savages with the hurling of stones and javelins and ending in modern ballistics with the great guns which were recently booming on the war fronts. Incidentally it may be noted that these stages are repeated with appropriate coenogenetic modifications in the ontogeny of the individual man. As a child he bites and scratches, as a boy he throws stones, or uses a sling or a shot-gun, and as an adult he sprays his real or fancied opponents and even his friends with showers of words, as I am doing this evening.

Although I have had to be very brief in my account of the evolution of the atomizer of *Formica rufa*, I believe enough has been said to show how limited would be our knowledge if we confined our attention to observations and experiments on this ant and how superficial is the viewpoint of the natural selectionist who would dispose of such a case by croaking the old formula: "If such a structure as the atomizer had not

appeared as a chance variation and been selected, the pismires would have gone to the wall in the struggle for existence." The case of the pismire is typical of many others in that it shows very clearly how the function guides and modifies and builds up the organ according to the principle of *Funktionswechsel*, first elucidated by Anton Dohrn. The function is continually changing, shifting, and dichotomizing in obedience to the needs and experience of the organism, and the organ merely reflects these changes in the development of its various parts. The atomizer of the pismire now subserves the function of creating around the nest a barrage of formic acid fumes, comparable to the poison gas and liquid fire of the German army, but it may truly be said also to epitomize the multimillennial history of such very diverse instruments as ambulatory limbs, an egg-laying machine, and a poisonous stiletto.

My second case is that of a peculiar fly, *Hilara sartor*, the male of which was long ago observed to fly about carrying in its claws a peculiar plaque of frothy white substance which it presented to the female and which was held by the latter during copulation. For some years this extraordinary performance furnished material for a rather acrimonious discussion among German and Austrian entomologists. In 1899 Aldrich and Turley observed another fly, since appropriately called *Empis aërobatica*, of the same natural family, the Empididæ, in the mountains of Idaho. The male of this species was seen to present the female with a beautiful frothy balloon, or diminutive Zeppelin, and closer observation showed that the gift bore at one end a minute dead fly of a different species. This behavior was if anything even more inexplicable than that of *Hilara sartor*, and speculation, pow-

erless to suggest its meaning, rested till Howlett and Hamm began to study the habits of various Empididæ in England. They found an extraordinary diversity in the behavior of these flies representing the following series of stages in the development of the instinct of *Hilara sartor*.

The first stage is seen in the ancestors of the Empididæ, the intrepid robber flies of the family Asilidæ, both sexes of which capture and devour insect prey before mating, because like some other adult insects, they need food in order to develop their eggs and sperm.

The second stage is seen in various species of *Empis*. According to Howlett the females of *E. borealis* dance up and down in the air in swarms till the males put in an appearance one by one, each bearing in his claws a recently killed fly of another species and hand it over to the female. The flies then settle on the vegetation in couples and copulation takes place while the females are diligently sucking the juices out of their wedding presents. These observations were later confirmed by Hamm, the assistant of Professor Poulton at Oxford, on various species of *Empis*, *Pachymeria*, and *Rhamphomyia*.

The third stage is represented by three species of *Hilara* (*maura*, *interstincta*, and *aëronetha*) observed by Girschner and Mik. The male of these species envelops his prey with a delicate froth which is really his dried saliva and then hands it to the female. The male's mouth waters, so to speak, after he has captured a juicy fly, but he represses his desire to devour it and presents it to his mate enveloped in the frothy evidence of his self-control. This leads directly to the condition seen in *Empis aërobatica*, the male of which has

discovered that it is very easy to capture some weak little fly and by adding to it a great mass of his own frothy saliva to convert it into a very acceptable present.

The fourth stage, finally, that of *Hilara sartor*, is easily derived from that of *Empis aërobatica* since the male of this fly simply gives up the hunt for prey altogether and presents his mate with a mass of spit-bubbles.

But this is not the whole story. The racial history of the Empidid courtship instinct dichotomized at what I have called the second stage and ran off into an interesting sideline, first elucidated by Hamm in his study of certain common species of *Hilara*, long known to European and American entomologists. The males of these insects fly in swarms with a peculiar zigzag movement directly over the surface of rapidly flowing streams. It was supposed that the flies were merely celebrating a kind of bridal dance, but the English observer showed that they are really carefully scrutinizing the surface of the water for the bodies of small dead insects or even for minute particles of wood and leaves, and that as soon as such objects float within their reach, they eagerly seize them, carry them up into the air and hand them to the females, which then promptly submit to the nuptial embrace while they turn their presents over and over with their legs. Hamm tells me that the performance can be easily and spectacularly demonstrated by throwing a lot of small, white objects, such as the ray-florets of a daisy, on the surface of the stream. As soon as they pass under the swarm the flies pounce on them, fish them out of the water and bear them aloft to their females like a lot of banners. Then the couples settle down on the vegetation and begin the serious business of procreation.

The nuptial instinct here sketched in its various phases is unusually interesting because it shows how portions of the living and inorganic environment may be drawn into the vortex of the developing impulse or craving as if they were merely so many added organs and how these portions of the environment change and merge into such purely physiological functions as secretion. The picture is not that of a lot of discrete tropisms or reflexes glued together like a collection of material particles as the mechanists would have us believe, but of activities radiating from and sustained by the simple need of the female for the possession of some small object, originally necessary as food but in many species now required merely for the satisfaction it furnishes through the tactile sense, and the need of the male to procure such an object as an indispensable means to the alleviation of his sexual appetite. That Virgil's *varium et mutabile semper femina* is not strictly true and that the female of such a highly endowed mammal as man has a similar persistent instinct is only too apparent. Perhaps the cave women had nothing to do with the cave men till the latter brought them steaks of the aurochs or the mammoth. But we need not go so far back in history to find analogies. There are females in our midst whose coyness has been overcome by a lobster and champagne supper, or the present of a diamond ring, a motor-car, or a bank account, and in future an aëroplane or a Zeppelin may be necessary, as it is in *Empis aërobatica*. Some, however, have been known to succumb to such easily procured trifles as a bunch of violets or a lock of hair. And if the war continues much longer and males become very scarce, no presents will be required, and the final condition seen in certain male Empidids, which are accepted even

when they present themselves empty-handed, will, I surmise, be only too common.

My third case has often been considered by writers on instinct but never, to my knowledge, with becoming seriousness. In the higher Lepidoptera we find two peculiar methods of pupation, which may be illustrated by the milk-weed butterfly (*Anosia plexippus*) and the cabbage butterfly (*Pieris rapi*). The mature caterpillar of the former spins with its lower lip on the under surface of a milk-weed leaf a button of silk and hangs from it by means of the hind legs. Soon the skin splits along the back and the chrysalis wriggles out and, to avoid falling to the ground, clamps the larval skin between two of its abdominal segments, till it can disengage its caudal end and hook it into the silk button. Then the shriveled skin is released and drops away and the chrysalis hangs motionless and head downward from the button. The cabbage-butterfly caterpillar spins, in addition to the silken pad, a rope-like girdle around the middle of its body and attaches the ends to the surface on which the button was spun. This girdle serves to support the chrysalis, very much as a papoose is held to the back of an Indian squaw by a strap, while the larval skin is being sloughed and the anal hooks are inserted in the pad.

Now the making of these small silken attachments and the accompanying behavior is a true hapaxoræic instinct of the deferred type, since it is unique in the life of the insect, performed with the most consummate skill and without imitation or previous instruction. It is in fact typical of the class of instincts that have elicited both the admiration and the glib explanations of natural philosophers and selectionists, largely because neither observation nor experiment

throws any light on the historical signification of such structures and behavior. When we study the cases of *Anosia* and *Pieris* comparatively and historically, however, they are seen to represent the last, highly specialized stages of a very long history, the course of which can be traced back through the moths, the ancestors of the butterflies, and the caddice flies, which are the ancestors of the moths, to the ancient and primitive insects of the Carboniferous age.

There are, however, insects still living, that give us a fairly satisfactory picture of the early developmental stages of the spinning instinct. Every spring I notice that the Harvard faculty and students tread on great numbers of the imported ground-beetle, *Carabus nemoralis*, because it has not yet learned to keep off the pavements around the college yard. If instead of putting your foot on one of these beetles you pick it up tenderly and give it a little piece of fresh beefsteak, it will return the kindness by giving you a demonstration of the first step in the evolutionary process that has culminated in your best silk socks and neck-tie. The beetle will begin by pouring a lot of saliva over the beefsteak, and if you have it under a microscope you will notice that the muscle-striations in the meat soon vanish and that the mass becomes gelatinous and then deliquesces.[4] When it reaches this condition, which is brought about by a powerful proteolytic ferment in the saliva, the beetle swallows and assimilates the food that has thus been digested outside its

[4] The liquid poured from the mouth of *Carabus* is probably not saliva but gastric juice, so that for this hypothetical stage in the development of the spinning instincts I would substitute such an employment of the saliva as that exhibited by the Peripatus and certain larval Mycetophilids. The former catches its prey by spitting at it and entangling it in viscid saliva, and some of the latter spin glutinous webs of saliva for the same purpose.

body. This method of digestion is now known to be very general in both biting and sucking insects and the larvæ of the lower forms, after digesting the soft parts of their insect prey, stick the indigestible remains together with more viscid saliva and convert them into an overcoat for their own soft bodies. The viscid saliva is merely archæic silk and the salivary glands which produce it are actually on the road to becoming the highly specialized sericteries of moths like the silk worm.

The larvæ of caddice flies and lower moths have for millions of years been making most extraordinary cases for themselves by spinning together any small bits of matter in their envornment, such as sand-grains, pebbles, small sticks, and leaves. They begin to use their sticky saliva or silk in this manner as soon as they hatch and keep on building cases throughout life, discarding them from time to time and constructing larger ones to fit their growing bodies. This behavior can be easily studied under artificial conditions. One has only to push a caddice fly larva out of its case and place it in a jar of water containing small bits of glass, iron filings, bits of filter paper, etc. After wriggling about for a few minutes it sets to work collecting these fragments and spins them together with astonishing skill into a case of the form peculiar to its species but consisting of materials which it has never before encountered. The caterpillars of the lower moths, which are very closely related to the caddice flies, spin their own feces together in a similar manner or roll up leaves and stitch them together with silk. In more specialized moths the larva remains naked and retains its silk in the sericteries till maturity, when it spins leaves or other objects together and makes a single case, the

cocoon, in which it pupates. A further stage is reached in such highly developed species as the silkworm, which no longer incorporate extraneous materials but make a perfectly elliptical cocoon of pure silk. In some species this substance is spun in such a way as greatly to facilitate the emergence of the moth at one end of the cocoon. In all cases, however, the cocoon of pure silk is spun only by attaching its first threads to foreign bodies as if to recall the preceding phylogenetic stages in which portions of the environment were actually incorporated in the fabric. The cocoon of the silk-worm and its allies represents the acme of the spinning instinct, which in the lower butterflies enters on a period of involution. Some Hesperid caterpillars spin a flimsy and degenerate cocoon, while other primitive butterflies make a structure with a Y-shaped area of denser silk, to which the chrysalis attaches itself. The cocoon is then omitted and the Y alone survives and is separated into two masses of silk, one of which, corresponding to the branches of the letter, becomes the girdle of *Pieris*, while the stem of the Y contracts to form the button. Finally, in *Anosia* the girdle is omitted and the minute button, from which the chrysalis hangs, alone persists as the last vestige of the cocoon. In some insects, like the ants, a further condition, that of the complete suppression of the spinning instinct, may be reached simply by a progressive thinning out of the walls of the cocoon.

The foregoing history is very instructive because it is so complete and shows how an hapaxoræic instinct may arise from one which is originally repeated throughout a long period of the insect's life. The suppression of the spinning instinct till the close of larval life in the higher Lepidoptera

and the large amount of liquid silk thus accumulated in the sericteries, seem to have enabled the insect to make a single supreme and complicated effort that would otherwise have been impossible. I believe that all deferred and hapaxoræic instincts may have had a similar origin from activities originally spread over the whole life period or over a whole developmental instar. The way is thus open for the interpretation of such structures as the ovipositor of the female and the copulatory organs, or "lock and key" arrangements, as Cope and T. H. Morgan have called them, of both sexes. The selectionists and mutationists appeal with great gusto to such structures, because they are so wonderfully adaptive although used only once during the life-cycle of most insects. There can be little doubt, however, that the most ancient insects, like existing cockroaches and termites, were long-lived and oviposited and copulated repeatedly and not only once like their modern very highly specialized descendants. Hence individual experience and use and disuse may have had much to do with perfecting these so-called "passive adaptations." Similarly such phenomena as the permanent protective coloration of insects may be regarded as the stereotype, highly specialized end-stage of a more ancient ability actively to change color in response to color changes in the environment, an ability still possessed by some primitive insects like the grasshoppers and mantids, though much more pronounced in cephalopod mollusks, fishes, amphibia, and lizards.

Of course, I do not pretend that the historical method of studying instincts, as I have endeavored to illustrate it, is capable of yielding results of great precision or certainty. It has serious limitations, some of which are inherent in the

limitations of the living and fossil faunas accessible to us. Many of the most extraordinary instincts are exhibited only by isolated and specialized groups of species, and though we may be able to detect certain developmental tendencies within a group, it is sometimes impossible, and may always be impossible, owing to the extinction of the more primitive allied forms, to form any satisfactory conception of the origins or early stages of a particular instinct. To this class belong the fungus-growing instincts of the Attine ants and of certain genera of termites and the nest-spinning habits of certain tropical ants of the genera *Camponotus*, *Polyrhachis*, and *Oecophylla*. Other limitations are inherent in the method itself, which is of such a character as to require constant revision and considerable restraint and taxonomic information on the part of the one who employs it. It is, nevertheless, sufficiently valuable to merit more attention on the part of modern biologists, and especially of some of our students, whose intellects are obnubilated by the notion that biology begins and ends in physics and chemistry and that it is bad form to be able to recognize at sight more than fifteen animals and ten plants.

The third method that promises important results in the study of instinct is the psychopathic, for we have been taught to believe that the investigation of the pathological has a value second only to that of experiment. There is much to support this view. Boris Sidis says, when referring to the belief that the investigation of the normal precedes that of the pathological:

> This belief is erroneous and is only given credence to by people who have not thought much on the subject, and especially by those who belong to the so-called "new psychology" school.

As a matter of fact the investigation of the abnormal in scientific research precedes that of the normal. The investigation of the abnormal is one of the most potent instruments for new discoveries. The method of experimentation, the most powerful tool of modern science, is in fact the creation of the artificial conditions, in other words, the effecting of the abnormal states. Where the compound is complex, where the constituent facts and their relations are imperfectly or all but unknown and not therefore under control, the spontaneous occurrence of some anomaly ought to be greeted enthusiastically, as it displays the rôle played by the modified or excluded factor. This is specially true in the case of mental life, where the phenomena under investigation are the most complex in the whole domain of science, where a direct modification of the functioning mental activity is as a rule impossible without the production of some anomaly.

Similar considerations have led me recently to read some twenty volumes of psychoanalytic literature comprising the works of Freud, Jung, Brill, Adler, Ernest Jones, Ferenzci, Bjerre, and W. A. White, with the result that I feel as if I had been taking a course of swimming lessons in a veritable cesspool of learning. As I have not since had an opportunity to take a spiritual shower-bath you will understand why my remarks throughout this paper lack the customary refinement of a Sunday evening discourse.

I should, of course, be wandering entirely off my beat if I attempted seriously to discuss psychoanalysis, but I cannot refrain from recording a few personal impressions of what I believe to be one of the most extraordinary and far-reaching contributions to thought. Having had a fling at nearly all the types of biologists and at the non-biologists who have handled instinct, I now see my opportunity to get under the skin of the psychologists. After perusing during the past twenty years a small library of rose-water psycholo-

gies of the academic type and noticing how their authors ignore or merely hint at the existence of such stupendous and fundamental biological phenomena as those of hunger, sex, and fear, I should not disagree with, let us say, an imaginary critic recently arrived from Mars, who should express the opinion that many of these works read as if they had been composed by beings that had been born and bred in a belfry, castrated in early infancy, and fed continually for fifty years through a tube with a stream of liquid nutriment of constant chemical composition. To put it drastically, most of our traditional psychologies are about as useful for purposes of understanding the human mind as an equal number of dissertations on Greek statuary would be to a student eager for a knowledge of anatomy. Such a student at once learns that the object of his investigation, the human animal body, is very largely composed of parts offensive to the aesthetic sense, but this does not deter him from studying them as thoroughly as other parts. The typical psychologist, who might be expected to study his material in the same scientific spirit, does nothing of the kind, but confines his attention to the head and the upper extremities and drapes or ignores the other parts.

Now I believe that the psychoanalysts are getting down to brass tacks. They have discovered that the psychologist's game which seems to consist in sitting down together or with the philosophers and seeing who can hallucinate fastest or most subtly and clothe the results in the best English, is not helping us very much in solving the terribly insistent problems of life. They have had the courage to dig up the subconscious, that hotbed of all the egotism, greed, lust, pugnacity, cowardice, sloth, hate, and envy which every

single one of us carries about as his inheritance from the animal world. These are all ethically and aesthetically very unpleasant phenomena but they are just as real and fundamental as our entrails, blood, and reproductive organs. In this matter, I am glad to admit, the theologians, with their doctrine of total depravity, seem to me to be nearer the truth than the psychologists. I should say, however, that our depravity is only about 85 to 90%.

In nothing is the courage of the psychoanalysts better seen than in their use of the biogenetic law. They certainly employ that great biological slogan of the nineteenth century with a fearlessness that makes the timid twentieth-century biologist gasp. But making all due allowance for the extravagant statements of Freud and Jung and their disciples, any fair-minded student of human nature is compelled to admit that there is a very considerable residuum of accurate observation and inference in their accounts of the dream, of the perversions of the nutritive and sexual instincts, of the erotic conflicts and repressions, and of the surviving infantilisms. If Freud told us, as he probably would if he were here, that all of us who have been smoking this evening have merely been exhibiting a surviving nutritional infantilism with the substitution of cigars for our mothers' breasts, we should, of course, exclaim, like some New England farmer confronted with a wildly improbable statement, *Gosh!* But after all, is the substitution by a man of a roll of dried *Nicotiana* leaves for a woman's breast any more preposterous than the Empidid's substitution of a balloon of salivary bubbles for a juicy fly, or the substitution by the birds living near a certain village of watch-makers in France, of discarded watchworks for twigs in the construction of their nests?

To me one of the most striking indications that the psychoanalysts are on the right road is the fact that many of their theories have such a broad biological basis that they can be applied, *exceptis excipiendis*, to a group of animals so remote from man as the insects. This has not escaped Jung, who calls attention to the striking analogies between the nutritive caterpillar stage and human infancy, the chrysalis and the period of latency, and the imaginal butterfly and puberty in man. There are even cases of repression and sublimation as in the workers of social insects, and did time permit I could cite examples of multiple personality or of infantilisms, that is, larval traits which survive or reappear in the adults of many species. Insects undoubtedly sleep. Do they dream? If they do, what a pity that we shall never be able to apply the Freudian analysis to the dreams of that symbol of sexual repression and sublimation, the worker ant!

But these are trivial considerations. The great fact remains that the work of the psychiatrists is beginning to have its effect even on such hidebound institutions as ethics, religion, education, and jurisprudence, and that the knowledge that is being gained of the workings of our subconscious must eventually profoundly affect animal no less than human psychology, since the subconscious *is* the animal mind.

IV

THE TERMITODOXA, OR BIOLOGY AND SOCIETY [1]

Cette civilisation, la plus ancienne que l'on connaisse est la plus curieuse, la plus logique, la mieux adaptée aux difficultés de l'existence qui, avant la nôtre, se soit manifestée sur ce globe. A plusieurs points de vue, encore que féroce, sinistre et souvent répugnante, elle est supérieure à celle des abeilles, des fourmis et de l'homme même. . . .

Sans en excepter les abeilles et les fourmis, en ce moment il n'y a pas, je le repète, sur cette terre d'être vivant qui soit tout ensemble aussi loin et aussi près de nous, aussi misérablement, aussi admirablement, aussi fraternellement humain.

Nos utopistes vont chercher, aux limites où l'imagination se décompose, des modèles de sociétés futures, alors que nous avons sous les yeux qui sont probablement aussi fantastiques, aussi invraisemblables, et qui sait, aussi prophétiques que ceux que nous pourrions trouver dans Mars, Vénus et Jupiter. — MAETERLINCK, *La Vie des Termites*

JUST before the World War we seemed to be on the verge of startling revelations in animal behavior. "Rolf," the Ayrdale terrier of Mannheim, was writing affectionate letters to Professor William Mackenzie of Genoa, and the Elberfeld stallions were easily solving such problems in mental arithmetic as extracting the cube root of 12,167, to the discomfiture of certain German professors, who had never been able to detect similar signs of intelligence in their students. The possibilities of animal correspondence struck

[1] Read at the Symposium of the American Society of Naturalists, Princeton Meeting, December 30, 1919; published in the *Scientific Monthly*, February 1920, and reprinted in *Foibles of Insects and Men* (New York: A. A. Knopf, 1928); included here by permission of The Science Press and A. A. Knopf.

me as so promising that I longed to dispatch letters and questionnaires to all the unusual insects of my acquaintance. But dismayed at the thought of the quantity of mail that might reach me, especially from the many insects that have been misrepresented by the taxonomists or maltreated by the economic entomologists, I decided to proceed with caution and to confine myself at first to a single letter to the most wonderful of all insects, the queen of the West African *Termes bellicosus*. During the autumn of 1915 my friend Mr. George Schwab, missionary to the Kamerun, kindly undertook to deliver my communication to a populous termitarium of this species in his back yard in the village of Okani Olinga. He subsequently wrote me that my constant occupation with the ants must have blinded me to the fact that the termitarium, unlike the formicarium, contains a king as well as a queen, but that the *bellicosus* king was so accustomed to being overlooked, even by his own offspring, that he not only pardoned my discourtesy, but condescended to answer my letter. Mr. Schwab embarked for Boston in 1917. Off the coast of Sierra Leone his steamer was shelled by a German submarine camouflaged as a small boat in distress, but succeeded in escaping, and what would have been another atrocity, the loss of the king's letter, was averted. It runs as follows:

Dear Sir: Your communication addressed to my most gloriously physogastric consort, was duly received. Her majesty, being extremely busy with oviposition — she has laid an egg every three minutes for the past four years — and fearing that an interruption of even twenty minutes might seriously upset the exquisitely balanced routine of the termitarium, has requested me to acknowledge your expres-

sion of anxiety concerning the condition of the society in which you are living and to answer your query as to how we termites, to quote your own words, "managed to organize a society which, if we accept Professor Barrell's recent estimates of geological time, based on the decomposition of radium, has not only existed but flourished for a period of at least a hundred million years."

I answer your question the more gladly because the history of our society has long been with me a favorite topic of study. As you know, the conditions under which I live are most conducive to sustained research. I am carefully fed, have all the leisure in the world, and the royal chamber is not only kept absolutely dark and at a constant and agreeable temperature even during the hottest days of the Ethiopian summer, but free from all noises except the gentle rhythmic dropping of her majesty's eggs and the soft footfalls of the workers on the cement floor as they carry away the germs of future populations to the royal nurseries. And you will not wonder at my knowledge of some of the peculiarities of your society when I tell you that in my youth I belonged to a colony that devoured and digested a well-selected library belonging to a learned missionary after he had himself succumbed to the appetite of one of the fiercest tribes of the Kamerun. If I extol the splendid solutions of sociological problems by my remote ancestors, I refrain from suggesting that your society would do well to imitate them too closely. This, indeed, would be impossible. I believe, nevertheless, that you may be interested in my remarks, for, though larger and more versatile, you and your fellow human beings are after all only animals like myself.

According to tradition our ancestors were descended in

early Cretaceous times from certain kind-hearted old cockroaches that lived in logs and fed on rotten wood and mud. Their progeny, the aboriginal termites, although at first confined to this apparently unpromising diet, made two important discoveries. First, they chanced to pick up a miscellaneous assortment of Protozoa and Bacteria and adopted them as an intestinal fauna and flora, because they were able to render the rotten wood and mud more easily digestible. The second discovery, more important but quite as incidental, was nothing less than society. Our ancestors, like other solitary insects, originally set their offspring adrift to shift for themselves as soon as they hatched, but it was found that the fatty dermal secretions or exudates of the young were a delicious food, and that the parents could reciprocate with similar exudates as well as with regurgitated, predigested cellulose. Thenceforth parents and offspring no longer lived apart, for an elaborate exchange of exudates, veritable social hormones, was developed, which, continually circulating through the community, bound all its individuals together in one blissful, indissoluble, syntrophic whole, satisfied to make the comminution and digestion of wood and mud the serious occupation of existence, but the swapping of exudates the delight of every leisure moment. It may be said, therefore, that our society did not arise, like yours, from a combination of selfish predatism and parasitism but from a coöperative mutualism, or symbiosis. In other words, our ancestors did not start society because they thought they loved one another, but they loved one another because they were so sweet, and society supervened as a necessary and unforeseen by-product.

You will admit that no society could have embarked on its

career through the ages with more brilliant prospects. The world was full of rotten wood and mud, and no laws interfered with distilling and imbibing the social hormones. But in the Midcretaceous our ancestors struck a snag. Not only had all the members of society begun to reproduce in the wildest and most unregulated manner, but their behavior toward one another had undergone a deterioration most shocking to behold. The priests, pedagogues, politicians, and journalists, having bored their way up to the highest strata of the society, undertook to influence or control all the activities of its members. The priests tried to convince the people that if they would only give up indulging in the social hormones and confine themselves to a diet of pure mud, they would in a future life eat nothing but rose-wood and mahogany, and the pedagogues insisted that every young termite must thoroughly saturate himself with the culture and languages of the Upper Carboniferous cockroaches. Some suspected that the main value of this form of education lay in intensifying and modulating the stridulatory powers, but for several thousand years most termites implicitly believed that ability to stridulate, both copiously and sonorously, was an infallible indication of brain-power. The politicians and the journalists — well, were it not that profanity has been considered to be very bad form in termite society since the Miocene, I might make a few comments on *their* activities. Suffice it to say that they consumed even more cellulose than the priests and pedagogues and secreted such a quantity of buncombe and flapdoodle that they well nigh asphyxiated the whole termitarium. Meanwhile, in the very foundations of the commonwealth, anarchists, syndicalists, I. W. W., and bolsheviki were busy boring holes and filling them with dy-

namite, while the remainder of society was largely composed of profiteers, grafters, shysters, drug-fiends, and criminals of all sizes, interspersed with beautifully graduated series of wowsers, morons, feeble-minded, idiots, and insane. [At this point the king has introduced a rather trivial note on the word "wowser." This word, he says, was first employed by the termites of Australia but later adopted by the human inhabitants of that continent to designate an individual who makes a business of taking the joy out of life, one who delights in pouring cold water into his own and especially into other peoples' soup. The term appears to be onomatopœic to judge from a remark by one of our postcretaceous philologists who asserts that "whenever the wowser saw termites dancing, swearing, flirting, smoking, or over-indulging in the social hormones, he sat up on his hind legs, looked very solemn, swelled out his abdomen, and said 'WOW!' "]

To such depths, my dear sir, the letter continues, had termite society fallen in the Midcretaceous. The few sane termites still extant were on the point of giving up social life altogether and of returning to the solitary habits of the Palæodictyoptera, but a king, Wuf-wuf IV, of the 529th dynasty, succeeded in initiating those reforms which led our ancestors to complete the most highly integrated social organization on the planet. He has aroused the enthusiastic admiration and emulation of every sovereign down to the present time. I can best describe him by saying that in his serious moments he displayed the statesmanship of a Hammurabi, Moses, Solomon, Solon, and Pericles rolled into one and that in his moments of relaxation he was a delightful blend of Aristophanes, Lucian, Rabelais, Anatole France, and Bernard Shaw. This king had the happy thought to

refer the problems of social reform to the biologists. They were unfortunately few in number and difficult to find, because each was sitting in his hole in some remote corner of the termitarium, boring away in blissful ignorance of the depravity of the society to which he belonged. In obedience to the king's request, however, they were finally rounded up and persuaded to meet together annually just after the winter solstice for the purpose of stridulating about the relations of biology to society. After doing this for ten million years they adopted a program as elegant as it was drastic for the regeneration of termite society, and during the remaining fifteen million years of the Cretaceous they succeeded in putting their plan into operation. I can give you only the baldest outline of this extraordinary achievement.

Our ancient biological reformers started with the assumption that a termite society could not be a success unless it was constructed on the plan of a superorganism, and that such a superorganism must necessarily conform to the fundamental laws of the individual organism. As in the case of the individual, its success would have to depend on the adequate solution of the three basic problems of nutrition, reproduction, and protection. It was evident, moreover, that these problems could not be solved without a physiological division of labor among the individuals composing the society, and this, of course, implied the development of classes, or castes. Termite society was therefore divided into three distinct castes, according to the three fundamental organismal needs and functions, the workers being primarily nutritive, the soldiers defensive, and the royal couple reproductive. Very fortunately our earliest social ancestors had not imitated our deadly enemies, the ants, who went crazy

in the early Cretaceous on the subject of parthenogenesis and developed a militant suffragette type of society, but insisted on an equal representation of both sexes in all the social activities. Our society is therefore ambisexual throughout, so that, unlike the ants, we have male as well as female soldiers and workers. It was early decided that these two castes should be forbidden to grow wings or reproduce and that the royal caste should be relieved from all the labor of securing food and defending the termitarium in order to devote all its energies to reproduction. The carrying out of this scheme yielded at least two great advantages: first, the size of the population could be automatically regulated to correspond with the food supply, and second, the production of perfect offspring was greatly facilitated.

During the late Cretaceous period of which I am writing our practical geneticists, in obedience to a general demand for a more varied diet, made two important contributions to our social life. The plant breeders found that what was left of the comminuted wood after its passage through the intestines of the worker termites could be built up in the form of elaborate sponge-like structures and utilized as gardens for the growth of mushrooms. Cultivation was later restricted to a few selected varieties of mushrooms which the biochemists had found to contain vitamins that accelerated the growth of the tissues in general and of the spermatocytes and oöcytes in particular. And for this reason only the royal caste and the young of the other castes were permitted to feed on this delicious vegetable food. The animal breeders of that age made a more spectacular though less useful contribution when they persuaded our ancestors to adopt a number of singular beetles and flies and to feed and care for

them till they developed exudate organs. Owing to the stimulating quality of their exudates these creatures, the termitophiles, added much variety to the previously somewhat monotonous social hormones. This quality, however, made it necessary to restrict the number of termitophiles in the termitarium for the same reason that your society would find it advisable to restrict the cattle industry if your animal breeders had succeeded in producing breeds of cows that yielded highballs and cocktails instead of milk.

It is, of course, one thing to have a policy and quite another to carry it out. The anarchistic elements in our late Cretaceous society were so numerous and so active that great difficulty was at first experienced in putting the theories of the biological reformers into practise, but eventually, just before the Eocene Tertiary, a very effective method of dealing with any termite that attempted to depart from the standards of the most perfect social behavior was discovered and rigorously applied. The culprit was haled before the committee of biochemists who carefully weighed and examined him and stamped on his abdomen the number of his colloidal molecules. This number was taken to signify that his conduct had reduced his social usefulness to the amount of fat and proteids in his constitution. He was then led forth into the general assembly, dismembered, and devoured by his fellows.

I describe these mores reluctantly and very briefly because I fear that they may shock your sensibilities, but some mention of them is essential to an appreciation of certain developments in our society within recent millennia. So perfectly socialized have we now become that not infrequently a termite who has a slight indisposition, such as a

sore throat or a headache, or has developed some antisocial habit of thought, or is merely growing old, will voluntarily resort to the committee of biochemists and beg them to stamp him. He then walks forth with a radiant countenance, stridulating a refrain which is strangely like George Eliot's "O, may I join the choir invisible!" and forthwith becomes the fat and proteid "Bausteine" of the crowd that assembles on hearing the first notes of his petition. If you regard this as an even more horrible exhibition of our mores, because it adds suicide to murder and cannibalism, I can only insist that you are viewing the matter from a purely human standpoint. To the perfectly socialized termite nothing can be more blissful or exalted than feeling the precious fats and proteids which he has amassed with so much labor, melting, without the slightest loss of their vital values, into the constitutions of his more vigorous and socially more efficient fellow beings.

Now I beg you to note how satisfactory was our solution of the many problems with which all animals that become social are confronted. I need hardly emphasize the matter of nutrition, for you would hardly contend that animals that can digest rotten wood and mud, grow perennial crops of mushrooms on their excrement, domesticate strange animals to serve as animated distilleries, and digest not only one anothers' bodies but even one anothers' secretions, have anything to learn in dietetics or food conservation. Our solution of the great problems of reproduction, notably those of eugenics, is if anything, even more admirable, for by confining reproduction to a special caste, by feeding it and the young of the other castes on a peculiarly vitaminous diet and by promptly and deftly eliminating all abnormalities,

we have been able to secure a physically and mentally perfect race. You will appreciate the force of this statement when I tell you that in a recent census of the 236,498 individuals comprising the entire population of my termitarium, I found none that had hatched with more than the normal number of antennal joints or even with a misplaced macrochæta. The only anomaly seen was one of no social significance, a slightly defective toenail in three workers. Rigid eugenics combined with rigid enforcement of the regulations requiring all antisocial, diseased, and superannuated individuals promptly to join the choir invisible, at the same time solved the problems of ethics and hygiene, for we were thus enabled, so to speak, to ram virtue and health back into the germ-plasm where they belong. And since we thus compelled not only our workers and soldiers but even our kings and queens to be born virtuous and to continue so throughout life, the Midcretaceous wowser caste, finding nothing to do, automatically disappeared. The problem of social protection was solved by the creation of a small standing army of cool-headed, courageous soldiers, to be employed not in waging war but solely for defensive purposes, and the development on the part of the soldiers and workers of ability to construct powerful fortifications. It may be said that the formation of the soldier caste as well as the invention of our cement subway architecture — an architecture unsurpassed in magnitude, strength, and beauty, considering the small stature of our laborers and the simple tools they employ — was due to the repeated failures, extending over many million years, of our politicians to form a league of nations with our deadly enemies, the ants. After a recent review of the army and an inspection of the fortifications

of my termitarium, I agree with several of the kings of the present dynasty who believed that we ought really to be very grateful to our archenemies for their undying animosity.

Such was our society at the beginning of the Eocene, and such, with slight improvements in detail, it has remained for the past fifty million years, living and working with perfect smoothness, as if on carefully lúbricated ball-bearings. Nor does it, like human society, live and work for itself alone, but with a view to the increase and maintenance of other types of life on the planet. On our activities depend the rapid decomposition of the dead vegetation and the rapid formation of the vegetable mould of the tropics. We are so numerous and our operations of such scope that we are a very important factor in accelerating the growth of all the vegetation, not only of the dry savannahs and pampas but even of huge rain-forests like those of the Congo, the Amazon, and the East Indies. And when you stop to consider that the animal and human life of the tropics absolutely depends on this vegetation you will not take too seriously the reports of our detractors who are forever calling attention to our destructive activities. One author, I am told, asserts that certain South American nations can never acquire any culture because the termites so quickly eat up all their libraries, and another gives an account of a gentleman in India who went to bed full of whisky and soda and awoke in the morning stark naked, because the termites had eaten up his pyjamas. How very unfair to dwell on the loss of a few books and a suit of pyjamas and not even to mention our beneficent and untiring participation in one of the most important biocœnoses!

You will pardon me if after this hasty sketch of our history I am emboldened to make a few remarks about your society, and in what I say you will, I hope, make due allowance both for the meagerness of my sources of information and the limitations of my understanding. I must confess that to me your society wears a strangely immature and at the same time senile aspect, the appearance, in fact, of a chimera, composed of the parts of an infant and those of a white-haired octogenarian. Although your species has been in existence little more than one hundredth of the time covered by our evolution, you are nevertheless such huge and gifted animals that it is surprising to find you in so imperfect a stage of socialization. And although every individual in your society seems to crave social integration with his fellows, it seems to be extremely difficult to persuade him to abate one tittle of all his natural desires and appetites, and every individual resists to the utmost any profound specialization of his structure and functions such as would seem to be demanded by the principle of the division of labor in any perfect society. Hence all the attempts which your society is continually making to form classes or castes are purely superficial and such as depend on the accumulation and transmission of property, and on vocation. And owing to the absence of eugenics and birth-control, and to your habit of fostering all weak and inefficient individuals, there is not even the dubious and slow-working apparatus of natural selection to provide for the organic fixation of castes through heredity. So immature is your society in these respects that it might be described as a lot of cave-men and cave-women playing at having a perpetual pink tea or Kaffeeklatsch.

But the senile aspect of your society impresses me as even more extraordinary because our society — and the same is true of that of all other social insects — is perennially youthful and vigorous, owing to our speedy elimination of the old and infirm. And this brings me to a matter that interests me greatly and one on which I hope we shall have much further correspondence. To be explicit, it seems that though your society has no true caste system, it is, nevertheless, divided into what might be called three spurious castes, the young, the mature, and the aged. These, of course, resemble our castes only in number and in consisting of individuals of both sexes. They are peculiar in being rather poorly defined, temporary portions of the life-cycle, so that a single individual may belong to all of them in succession, and in the fact that only one of them, comprising the mature individuals, is of any great economic value to society and therefore actually functions as the host of the two others, which are, biologically speaking, parasitic. To avoid shocking your human sensibilities, I am willing to admit that both these castes may be worth all the care that is bestowed on them, the young on account of their promise and the old on account of past services. And I will even admit the considerable social value of the young and the old as stimuli adapted to call forth the affection of the mature individuals. But, writing as one animal to another, I confess that I am unable to understand why you place the control of your society so completely in the hands of your aged caste. Your society is actually dominated by the superannuated, by old priests, old pedagogues, old politicians, and no end of old wowsers of both sexes who are forever suppressing or regulating everything from the observance of the Sabbath and the wear-

ing of feathers on hats to the licking of postage stamps and the grievances and tribulations of stray tom-cats.

I notice that your educators, psychologists, and statisticians have much to say on human longevity, and you seem all to crave for nothing so much as an inordinate protraction of your egos. Psychologically, this is, of course, merely another manifestation of your fundamentally unsocial and individualistic appetites. Your writers make much of your long infancy, childhood, and adolescence as being very conducive to educability and socialization, and this is doubtless true, but the fact seems to be overlooked that the great lengthening of the initial phases of your life-cycle is also attended by a grave danger, for it also increases the dependence of the young on the adult and aged elements of society, especially on the parents, and this means intensifying what the Freudian psychologists call the father and mother complexes and therefore also an increased subservience to authority, a cult of the conservative, the stable and the senile. The deplorable effects of intensifying these complexes have long been only too evident in your various religious systems and are already beginning to show in the all too ready acceptance on the part of your society of the visionless policies and confused and hesitating methods of administration of your statesmen.

Unless I am much mistaken this matter of the domination of the old in your society deserves careful investigation. Unfortunately very little seems to be known about senility. In our society it cannot be investigated, because we do not permit it to exist, and in your society it is said to be very poorly understood, because no one is interested in it till he actually reaches it and then he no longer has the ability or

the time to investigate it. When the social significance of this stage in the human life-cycle comes to be more throughly appreciated some of your young biologists and psychologists will make it a subject of exhaustive investigation and will discover the secret of its ominous and persistent domination. It will probably be found that many of your aged are of no economic importance whatever, and that the activities of many others may even be mildly helpful or beneficial, but you will find, as we found in the Midcretaceous, a small percentage, powerful and pernicious out of all proportion to their numbers, who are directly responsible for the deplorable inertia of your institutions, especially of your churches, universities, and political bodies. These old individuals combine with a surprising physical vigor a certain sadistic obstinacy which consecrates itself to obstructing, circumventing, suppressing, or destroying not only everything young or new, but everything any other old individual in their environment may suggest. The eminent physician who recommended chloroform probably had this type of old man in mind. Certain economic entomologists have advocated some more vigorous insecticide, such as hydrocyanic acid gas. This is, however, a matter concerning which it might be better to defer recommendation till the physiology, psychology, and ethology of the superannuated have been more thoroughly investigated.

It has sometimes occurred to me that your social problem may be quite insoluble — that when your troglodyte ancestors first expanded the family and clan into society they were already too long-lived, too "tough," and too specialized mentally and physically ever to develop the fine adjustments demanded by an ideal social organization. I feel cer-

tain, nevertheless, that you could form a much better society than the present if you could be convinced that your further progress depends on solving the fundamental, preliminary problems of nutrition, reproduction, and social defence, which our ancestors so successfully solved in the late Cretaceous. These problems are, of course, extremely complicated in your society. Under nutrition you would have to include raw materials and fuel, that is, food for your factories and furnaces as well as food for your bodies. Your problems of reproduction comprise not only those of your own species but of all your domesticated animals and plants, and your social defence problems embrace not only protection from the enemies of your own species (military science) but from the innumerable other organic species which attack your domesticated animals and plants as well as your own bodies (hygiene, parasitology, animal and plant pathology, economic entomology). Like our ancestors you will certainly find that these problems can be solved only by the biologists — taking the word "biologists" in its very broadest sense, to include also the psychologists and anthropologists — and that till they have put their best efforts into the solution your theologians, philosophers, jurists, and politicians will continue to add to the existing confusion of your social organization. It is my opinion, therefore, that if you will only increase your biological investigators a hundred fold, put them in positions of trust and responsibility much more often and before they are too old, and pay them at least as well as you are paying your plumbers and bricklayers, you may look forward to making as much social progress in the next three centuries as you have made since the Pleistocene. That some such opinion may also be entertained

by some of your statesmen sometime before the end of the present geological age, is the sincere wish of

Yours truly,

WEE-WEE

43rd Neotenic King of the 8429th
Dynasty of the Bellicose Termites.

On reperusing this letter before deciding, after many misgivings, to read it to so serious a body of naturalists, I notice a great number of inaccuracies and exaggerations, attributable, no doubt, to his majesty's misinterpretation of his own and very superficial acquaintance with our society. His remarks on old age strike me as particularly inept and offensive. He seems not to be aware of the fact that at least a few of our old men have almost attained to the idealism of the superannuated termite, a fact attested by such Freudian confessions as the following, taken from a letter recently received by one of my colleagues from a gentleman in New Hampshire:

I do not understand how it is that an insect so small as to be invisible is able to worry my dog and also at times sharply to bite myself. A vet. friend of mine in Boston advised lard and kerosene for the dog. This seemed to check them for a time, but what I need is extermination, for I am in my eighty-fourth year.

V

THE ORGANIZATION OF RESEARCH[1]

First of all, it is necessary to rid ourselves once and for all of the notion that organization is in itself a good thing. It is very easy to fall into the notion that growing complexity is a sign of progress, and that the expanding organization of Society is a sign of the coming of the Cooperative Commonwealth. . . . Organization is a marvelous instrument through which we every day accomplish all manner of achievements which would be inconceivable without it; but it is none the less better to do a thing without organization if we can, or with the minimum of organization that is necessary. . . .

In complex modern communities there are so many things that must be organized that it becomes more than ever important to preserve from organization that sphere in which it adds least to, and is apt to detract most from, our field of self-expression — the sphere of personal relationships and personal conduct. — G. D. H. Cole, *Social Theory*

BEFORE delivering my paper I wish to confess that I find myself in a somewhat unpleasant predicament, for when I began it and even after sending its title to Professor Allee I was of the opinion that research might, perhaps, be amenable to organization, but after thinking the matter over I was compelled to reverse my opinion, with the result that what I shall say may strike some of you as painfully reactionary. Still I encouraged myself with the reflection that many others have written papers with misleading titles and that I might perhaps put much of the blame for the results on my confrères of Section F for con-

[1] Address of the retiring vice-president and chairman of Section F — Zoological Sciences — American Association for the Advancement of Science, Chicago, 1920; published in *Science*, N.S., vol. LIII (January 21, 1921) and reprinted in *Foibles of Insects and Men* (New York: A. A. Knopf, 1928); included here by permission of The Science Press and A. A. Knopf.

ferring so signal an honor as its chairmanship on one of its tired old bisons from the taxonomic menageries instead of on one of its fresh young bulls from the Mendelian byre. I might say also, in further justification of myself, that I at least selected the most fashionable and exalted topic I could find, for you must all have observed that at the present time no word occurs with greater frequency and resonance in serious discourse than "organization." Everybody is so busy organizing something or inciting some one to organize something that the word's subtly concealed connotations of control and regulation appear to be overlooked. The purpose of organization is instrumental, as is shown by the derivation of the word, from "organon," a tool, or implement, which is in turn derived from "ergo," to work. It is one of those superb, rotund words which dazzle and hypnotize the uplifter and eventually come to express the peculiar spirit or tendency of a whole period.

These words, which for want of a better term I may call "highbrow," and the conceptions they embody, are so interesting that I will dwell on them for a moment. During the late Victorian period the most highbrow word was "progress." It disappeared and gave place to "organization" with the World War, when we realized that the evolution of our race since the Neolithic Age was not nearly as substantial as we had imagined. Neither the Greeks nor the people of the Middle Ages seem to have had either of these words or their conceptions, though the Greeks, at least, did a fair amount of progressing and organizing.[2] The Mediæval

[2] "The notion of cosmic progress was foreign to the Oriental, Greco-Roman, and Christian worlds, in which prevailed from time to time beliefs in cycles or of a return to a primitive state or of a lapse from an original

highbrow words were "chivalry" and "honor," the latter persisting down to the present day in Continental Europe in the German students' dueling code, as a living fossil, or what biologist would call a "relict." Schopenhauer [3] remarked that the duel and venereal diseases were the only contributions to culture the race had made since the classical period, overlooking the fact that the Greeks and the Japanese had their own highbrow words and institutions. Gilbert Murray [4] has shown that the word *aidos*, which the Achæan chiefs of the Homeric age so solemnly uttered, was applied to a peculiar kind of chivalry, and the *bushido* of the Japanese was another similar though independent invention. All of these conceptions — progress, organization, chivalry, aidos, bushido — seem to start among the intellectual aristocracy and all imply a certain *noblesse oblige*, for there is no fun in continually exhorting others to progress unless you can keep up with the procession, or of organizing others unless you yearn to be organized yourself, just as there is no fun in getting up a dueling or bushido code unless you are willing to fight duels or commit hara-kiri whenever it is required by the rules of the game.

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state of perfection. In its modern form the notion of progress had its origin among thinkers freed from religious preconceptions and addicted for one reason or another to philosophies of becoming or evolution." B. Croce, *Filosofia della Pratica*, 3d ed. (Bari: G. Laterzi & figli, 1923), p. 188.

[3] "Zwei Dinge sind es hauptsächlich, welche den gesellschaftlichen Zustand der neuen Zeit von dem des Alterthums, zum Nachtheil des ersteren unterscheiden, indem sie demselben einen ersten, finstern, sinistern Anstrich gegeben haben, von welchem frei das Alterthum heiter und unbefangen, wie der Morgen des Lebens, dasteht. Sie sind: das ritterliche Ehrenprincip und die venerische Krankheit — *par nobile fratrum*!" Schopenhauer, *Parerga und Paralipomena*, in *Sämmtliche Werke*, ed. Frauenstädt (Leipzig, 1888), v, 413.

[4] *The Rise of the Greek Epic*, 2d ed. (Oxford: The Clarendon Press, 1911), pp. 103–112.

Of course, the vogue of "organization" was abnormally stimulated by the mobilization of armies and resources for the World War. We acquired the organizing habit with a vengeance and have not since had time to reflect that there may be things in the world that it would be a profanation to organize — courtship, for example — or not worth organizing — a vacuum, for example — or things that cannot be organized, or if organizable, better left as they are — scientific research, perhaps.

There are at least three different types of organization. One of them we find ready to hand in individual animals and plants, in our own bodies and in animal colonies and societies, that is to say, in complexes which organize themselves both onto- and phylogenetically. This is a self-contained type of organization, requiring much time and energy for its consummation and though very intricate and profound still sufficiently plastic and adaptable to trade with time and the environment and to resist a considerable amount of thwarting and meddling. For obvious reasons this type appears to us to be so admirable that it influences all our conceptions of organization. If the Greeks had coined a word for organization — the nearest word, orgánosis, seems not to appear till the twelfth century — they would probably have applied it to a second type of cases, in which an agent organizes a complex as an engine for accomplishing certain results. In this sense Mr. Ford would be an organizer of motor cars, and in such a sense theologians might speak of the Deity as organizing the universe. This is organization imposed on inorganic or at any rate alien materials. At the present day the word is not used in this sense, since the notion of life in the materials to be organized seems

to be so essential. There is, however, a third type, which is intermediate between the two preceding, one in which certain elements of a living complex are permitted or delegated or arrogate to themselves the right to organize the remaining elements, as is seen in innumerable human organizations from a state, church, or army to a band of robbers. This type of organization can often be swiftly accomplished, especially if reinforced by the first type, but is necessarily more or less of an artifact and prone to easy and unexpected disintegration. We have this type in mind when we speak of the organization of scientific research, or investigation.

It is evident, moreover, that the organization of research up to the present time has developed according to the first type, through a natural division of labor and inclination among investigators and by means of such coöperative *liaison* agencies as learned societies and publications. Even the most pessimistic among us must be lost in admiration at the results thus accomplished during the past few centuries. But the organizers feel that we have been moving too slowly and have been wasting too much time and effort — and they also feel, apparently, that natural, or organic organization of research, like that of the past, affords too little scope for the expression of those instincts of self-assertion and domination which are so evidently associated with the accumulation of hormones in the older males of all mammals. These hormones commonly produce such an obfuscation of the intellect that even our mature biologists seldom realize that they are headed for the fate of the old rogue elephants and bulls, which, when they try to do too much organizing, are promptly and unceremoniously butted out of the herd by the youngsters.

The phrase "organization of research" is nonsense if we take "research" in its abstract sense, for an abstraction, of course, is one of the things that cannot be organized. All we can mean by the term is the organization of the actual processes of research, or investigation, and since these processes are essentially nothing but the living, functioning investigators themselves, organization of research can mean only the organization of the investigators. It would seem desirable, therefore, before attempting such organization to make a behavioristic study of these creatures — either to catch and closely observe a number of them or to steal on them unawares while they are in the full ardor of research — in other words to investigate the investigators. Unfortunately no one has made such a study, which should, of course, precede the making of a card catalogue of the various species, subspecies, varieties, mutations and aberrations of investigators and the enumeration of their genes and chromosomes. And as the investigators themselves seem to be so busy that they have no time to scrutinize their own behavior, or if they do, are either too proud or too bashful to tell us what they find, I am compelled, for the sake of my argument, to attempt such a study and hence to make a brief excursion into psychology. As this is one of the fields in which it is still possible to do a certain amount of loose thinking with impunity, I may hope to return sufficiently intact to proceed with the discussion.

It is often supposed that the investigator enters his laboratory full of instruments and glassware and proceeds, with the use of this equipment, his sense organs, and his carefully controlled ratiocinative powers to excogitate the discoveries which our newspaper editors occasionally deign to distort for

the benefit of the readers of their Sunday supplements. But every investigator who observes his own activities or those of other investigators knows that this is, to say the least, a very inadequate account of the process, and every psychologist knows that while the proper employment of the senses and the reasoning powers is extremely important, the real "drives" are the instincts, emotions and interests, or what some authors prefer to call in more anaemic terms, the propensities, conative tendencies, sentiments, or dispositions. To the biologist, who takes a behavioristic view of the instincts, it is difficult to single out the various drives that initiate, determine and sustain such intricate activities as those leading to scientific discovery and invention, and the psychologists themselves are far from unanimous on this matter. The list submitted in the sequel is, therefore, merely an approximation to the true state of affairs, though it is probably adequate for the purpose I have in mind.

To merit the designation of human instincts, in the conventional sense, tendencies or dispositions must be innate and purposive, common to all the normal individuals of our species, less overlaid or camouflaged by habits and therefore more evident in the young than in the adult, and represented by similar though more rudimentary tendencies in the higher mammals. Such instincts seem to be rather numerous and several of them are exhibited by the investigator in a highly specialized form or are at any rate evoked and conditioned by very specific objects or situations. We can recognize:

1. Curiosity, which seems to be clearly manifested in many mammals, like the cow which stares at us across the pasture, and in the open-mouthed wonder of the child. It is so characteristic not only of individuals but of whole

peoples that the Germans often refer to it as a national peculiarity of the Saxons. In the investigator it is commonly insatiable and very intense, because restricted to certain objects and relations, particularly to the causal relations among phenomena. Its importance has been noticed by many writers. McDougall[5] says that in men in whom curiosity is innately strong, "it may become the main source of intellectual energy and effort; to its impulse we certainly owe most of the purely disinterested labors of the highest types of intellect. It must be regarded as one of the principal roots of both science and religion." It is perhaps worthy of note that "inquiry" is often used as a synonym of investigation, and that any problem is most naturally and most concisely stated in the form of an interrogatory sentence.

2. The hunting instinct, which is primarily nutritive in animals and remains so very largely in savages. In children and adults of civilized man it persists in the form of sport and the love of rapid movement in such intensity that it is leading to the extinction of our native faunas and an enormous development of the automobile industry, while in the investigators — the word itself means followers of an animal's spoor — such as zoologists, archeologists, and explorers it is too apparent to require discussion. It is not lacking, however, in other investigators, all of whom when too old or too lazy to hunt their accustomed prey in the open, delight to sit and hunt for the opinions of others and especially for confirmation of their own opinions, in comfortably heated libraries.

3. The acquisition, collecting, or hoarding instinct, also

[5] *An Introduction to Social Psychology* (Boston: Luce & Co., 1910), p. 59.

primarily nutritive in animals and savages, but modified in children and adults of civilized peoples, in whom it manifests itself in the most extraordinary form of amassing all sorts of objects, from newspaper clippings and cigar-bands to meerschaum pipes and shaving mugs. It is unnecessary to dwell on its truly monomaniacal manifestations among zoologists and botanists, who collect everything from mites to whales and from bacteria to sequoias. But even those who look down with contempt on the enthusiastic collectors of bird-lice or coprolites are themselves usually addicted to collecting so-called data or statistics. The significant difference between the mere magpie-like collector and the hamster-like investigator lies, of course, in the use made of the accumulated objects.

4. The instinct of workmanship, craftsmanship, or contrivance, which also has its phylogenetic roots in the constructive activities of very many animals. In man it begins ontogenetically with the making of mud-pies and may lead to such achievements as the excavation of the Panama Canal or the construction of an airship. It is, as Veblen [6] and others have shown, an instinct of the greatest importance. In the investigator it is seen in the inventing of methods and devices and the construction of apparatus and hypotheses, and reaches its highest manifestations in flights of the creative imagination.

The four instincts I have been very briefly considering might be called individual to distinguish them from four others which are more deeply rooted in the social life of the investigator. These are:

5. Emulation. The decision as to whether this may be

[6] *The Instinct of Workmanship* (New York: B. W. Huebsch, 1918).

traced among animals to competition for food or for mates may be left to Jung and Freud and their respective disciples. According to William James,[7] emulation is "a very intense instinct, especially rife with young children or at least especially undisguised. Every one knows it. Nine tenths of the work of the world is done by it. We know that if we do not do the task some one else will do it and get the credit, so we do it." It is powerful and elaborately conditioned in investigators and perhaps the less said about it the better. The word "priority" will conjure up in your minds a sufficient number of emotionally toned ideas to meet the needs of this discussion.

6. What for lack of a better term I shall call the instinct of communication. It seems to have its roots in the behavior of those more or less gregarious or social animals, which apprise one another by signs or sounds of the presence of danger, of food or of certain sexual states. Its manifestations may be said to range from the chirping of crickets, tree frogs, and birds to the invention of language and the effusions of poetry and music, both vocal and instrumental. In both the old and the young of our species it appears also as the by no means sex-limited impulse to gossip and divulge secrets, to communicate news and rumors, much information, and no little misinformation. It urges the investigator to communicate the results of his activities to learned societies and to publish those results to the world or at least to a select coterie of specialists. The strength of this instinct might be tested by passing stringent laws forbidding certain investigators from attending scientific meetings or publishing anything

[7] *The Principles of Psychology* (New York: Henry Holt & Co., 1890), II, 409.

for long periods of time or during their life-time or even posthumously. The results of such experimental repression might be illuminating but I refrain from speculating on their nature.

7. Closely connected with this instinct of communication is the craving for sympathy and appreciation so clearly exhibited by most highly social animals and so undisguisedly shown by children. Most investigators exhibit such a moderate development of this craving that they seem to be quite satisfied with the good opinion of the workers in their own specialties. But even if more appreciation were demanded the individual investigator would stand little chance of obtaining it, for investigators have become so numerous and the field of their labors has been so vastly expanded through their own enthusiastic efforts and so thickly overgrown with a dense crop of technicalities of their own sowing and cultivation, that most of them can be known only to those who are working in the same or adjoining furrows.

8. The instinct of coöperation — also very evident and of far-reaching significance in gregarious and social animals and manifested in the team-play of young human beings and the innumerable associations of adults. In many investigators this instinct seems to be rather feeble but may still appear at least in the ambition to figure in the role of an honest hod-carrier in the erection of some small fragment of the great edifice of human knowledge. In others it may be sufficiently developed to constitute a powerful drive to the invention of labor-saving devices and machinery, methods of preventing disease and increasing longevity and mental and physical efficiency.

This list is probably incomplete, but I believe that it com-

prises at least the more important drives of the investigator. The special trend of his activities is, no doubt, further determined by his native capacities, but the psychological problem as to whether or not these also constitute drives, as Woodworth [8] maintains and McDougall [9] denies, I shall not attempt to discuss. The point I wish to emphasize is that the specific activities of the investigator depend primarily and preëminently on his instincts, emotions, interests, and native endowments.

If we turn now to a survey of investigators in general we find that they can be divided into two classes, usually called theoretical and practical, or pure and applied. The term pure is, to say the least, somewhat priggish, since it seems to imply that its alternative is more or less contaminated, and theoretical and practical are unsatisfactory because all investigation is necessarily both. I prefer, therefore, to designate the two classes as discoverers and inventors, since the former are primarily interested in increasing our knowledge of our environment and of ourselves, the latter in increasing our power over our environment and ourselves. From the very nature of this distinction it follows that the discoverer pursues more general, more theoretical, and therefore more remote aims, whereas the inventor, in the very broad sense in which I am using the term, busies himself with more special, more practical, and therefore more immediate problems. As both types of investigation are equally essential to the fullest spiritual and economic exploitation of the universe, no society can attain to a high level of culture unless

[8] *Dynamic Psychology* (New York: Columbia University Press, 1918), pp. 66 ff.

[9] "Motives in the Light of Recent Discussion," *Mind*, N.S., XXIX, 277–293 (1920).

it provides impartially both for its discoverers and its inventors.

There is another classification of investigators which will be useful for the purposes of my argument — namely, into professionals and amateurs. I am, of course, using these words in their good sense, not with the evil connotations that have grown up around them. It is clear that both may suffer from certain disabilities, the professional from well-known guild restrictions, the amateur from lack of opportunity or equipment or of the lively interchange of ideas so necessary to the most fruitful type of investigation. Both, too, have their advantages, the professional in the support and advertisement of his guild-fellows, the amateur in the freedom to choose and delimit his own problems, to work on them in his own way, and to publish when he sees fit. These distinctions did not escape that clever old fox, Samuel Butler, who says:

> There is no excuse for amateur work being bad. Amateurs often excuse their shortcomings on the ground that they are not professionals, the professional could plead with greater justice that he is not an amateur. The professional has not, he might well say, the leisure and freedom from money anxieties which will let him devote himself to his art in singleness of heart, telling of things as he sees them without fear of what man shall say unto him; he must think not of what appears to him right and lovable but of what his patrons will think and of what the critics will tell his patrons to say they think; he has got to square every one all round and will assuredly fail to make his way unless he does this; if, then, he betrays his trust he does so under temptation. Whereas the amateur who works with no higher aim than that of immediate recognition betrays it from the vanity and wantonness of his spirit. The one is naughty because he is needy, the other from natural depravity. Besides the ama-

teur can keep his work to himself, whereas the professional man must exhibit or starve.[10]

Contrasting the professional and amateur, to the advantage of the latter, was also a favorite pastime with that irritable old bear, Schopenhauer. He compared the professionals with dogs, the amateurs with wolves, but he was not always consistent zoologically, for he sometimes thought of the professionals as cattle, as, for example, when he says, "On the whole, the stall-feeding of our professorships is most suitable for ruminants, but those who receive their prey from the hands of Nature, live best in the open."[11]

At present the terms professional and amateur seem to have fallen into disuse among scientists, for reasons that are not far to seek. We know that during the eighteenth and nineteenth centuries, when the books and apparatus necessary for the prosecution of research were so meager as to be within the reach of men of very moderate means, amateurs were able to do a vast amount of important work in all the departments of science. This was particularly true in England and America. In England we have a teacher of music, William Herschel, making great discoveries in astronomy; a stone-cutter, Hugh Miller, in geology; a Nottingham cobbler, George Green, in mathematics; a grocer of Ightham, Harrison, and a jeweler of St. Leonards, W. J. L. Abbott, in archeology, and a country gentleman, Charles Darwin, in biology. There were men like John Hunter, Lyall, Wallace, Galton, Samuel Butler, Lubbock, Bates, and a host of other eminent investigators, who really be-

[10] *The Notebooks of Samuel Butler*, ed. by H. F. Jones (New York: E. P. Dutton & Co., 1917), p. 145.

[11] *Op. cit.*, VI, 519.

longed to the class of amateurs. Till very recently whole sciences, such as taxonomy and zoogeography, entomology and genetics, were almost entirely in the hands of amateurs. Mendel was an amateur, and all the wonderful varieties of our domestic animals and plants were developed, one might almost say invented, by amateurs. The change which has come over the situation is due to the great increase in our knowledge in more recent times and the exuberant growth of our universities, technical schools, museums, and research institutions. These have made investigation more and more difficult for the amateur, especially in the inorganic sciences and in physiology, which now demand an exacting preparation and elaborate apparatus, although there are even at the present time a few eminent amateur astronomers and geologists. Amateurs still abound, nevertheless, in zoology and botany, in which it is still possible to carry on much valuable research with very simple equipment. There must be thousands of them, and nothing is more extraordinary than the ignorance of their work on the part of many of our university professionals. I could give a long list of men in the most diverse professions, lettercarriers, stage-coach drivers, hosiers, portrait-painters, engravers, parsons, priests, stock-yard superintendents, engineers, bankers, country grocers, country doctors, army officers, mining prospectors, school-teachers, and clerks, whose researches have greatly enriched entomology and other departments of zoology. In such vast and complicated sciences as biology and archeology the work of the amateur is so much needed and so worthy of encouragement that we may regard it as one of the greatest defects of our educational system that a youth is ever able to leave the science courses of a high school or college and

take up the humblest calling without a fixed determination to fill at least a portion of his leisure hours with the joys of research.

The disuse of the words professional and amateur is also, no doubt, due to the fact that the two kinds of investigators can no longer be sharply distinguished. Not only are the biologists in our universities and museums frequently recruited from the ranks of the amateurs, but as investigators in those institutions many of them remain amateurs in spirit and merely exercise the teaching and curatorial professions because they can be more conveniently carried on in conjunction with research than more lucrative professions such as undertaking and plumbing. There is no reason to suppose that the number of amateur investigators may not greatly increase under a more favorable form of society. In the ideal commonwealth of the future it may not be in the least surprising to find that the communal furnace-man, after his four-hour day, is conducting elaborate investigations in paleobotany, and that the communal laundress is an acknowledged authority on colloidal chemistry.

Now if the preceding very hasty behavioristic account is accurate we must admit that it would be difficult to find a body of men more unfavorable for purposes of organization, even by a committee of their own class, than the investigators. Many reasons might be given in support of this statement, but I shall consider only the following four:

1. The activities of the investigator depend, as we have seen, on an array of instincts, emotions, and interests, many of which are so positive that their organization in the sense in which organizers are using the term is out of the question. It is possible, of course, to overstimulate, repress, per-

vert, and exploit instincts, and they are undoubtedly able to organize themselves by long processes of interplay, mutual adjustment, and coördination, but even regulation of them *ab extra* is exceedingly difficult. In this matter the experience of the race in its age-long endeavors to regulate and organize such powerful drives as the sexual and parental instincts should be sufficiently illuminating, and the instincts of the typical inventor and discoverer seem to be every bit as imperative. The impossibility of organizing even a small body of investigators can be easily tested. Such bodies exist in our large universities, very small in comparison with the total number of investigators in the country, but large enough, if organized, to determine and control the whole policy of their respective institutions. But if any investigator attempts to organize such a body for such a purpose or for any other of mutual advantage, he will at once find his efforts frustrated or at any rate circumvented, by a lot of individuals, turgid with peculiar instincts, emotions, and purely personal interests and as blind to their collective interests as an equal number of soft-shell clams. Furthermore, it is important to note that the difficulties of organizing are greatly increased by the skeptical and critical attitude of mind which the investigator is bound to cultivate and the defective development of certain dispositions in his constitution, such as the gregarious instinct and the instinct of self-abasement and susceptibility to suggestion, propaganda, and leadership, which render other men so prone or at least so accessible to social, religious, and political organization.

2. Attempts at organizing investigators must fail because their highly specialized activities depend to such a great extent on their peculiar native aptitudes or capacities. The

organizers are willing to admit that they are baffled by the geniuses, but these are dismissed as very rare birds, notwithstanding the fact that their influence on the trend of scientific research is out of all proportion to their numbers. The great majority of investigators appear on superficial acquaintance to be such commonplace, unassuming specimens of humanity that it would seem that they and society in general could only be greatly benefited by having their problems "assigned" and their investigative efforts directed, controlled, and organized. This notion seems to me to be due to a singularly defective insight into the peculiar psychology of investigators. No one who has had long and intimate relations with these men can fail to be impressed with the extraordinary diversity of their aptitudes, and nothing is more evident than that these aptitudes must be permitted to express themselves not only with the greatest freedom, but even in the most whimsically personal manner. Nor can any one who is running a laboratory fail to notice that he can secure the fullest enthusiasm, devotion, and team-play from all his men only on the condition that all considerations are absolutely subordinated to the ideals of research. He knows that some investigators can do their work best with a slow, uniform, and apparently never-tiring motion, others with a ravenous, carnivore-like onrush, accompanied by an expenditure of vitality so magnificent that they have to loaf for a considerable period before they can store sufficient energy for another onslaught on their problem, and that there are many others whose investigative activities are of an intermediate and more evenly rhythmical type. Yet men of such diverse aptitudes and habits of work can be easily induced to live in harmony and accomplish

much valuable work if any suggestion of such things as punctuality, punching time-clocks, and other efficiency and factory devices are most carefully avoided. So sensitive is the investigator to the need of giving expression to his capacities and of doing his work in his own way that any one who is enough of a martinet to insist on introducing any of the devices to which I have alluded will at once build up a defense reaction sufficiently powerful to vitiate or inhibit all the research activities of his laboratory. It is for this reason, I believe, that even the vague, tentative suggestions of the organizers are already creating a resentment or at any rate a resistance that would surprise no one who is not bent on behaving like the proverbial bull in a china shop.

3. Whatever may be the value of research to the individual investigator, it is certain that its only social value lies in the discoveries and inventions to which it may lead. The investigative genius may be defined as one who is in a chronic state of discovery or invention, whereas the ordinary investigator approximates genius more or less closely according to the frequency of his creative achievements. Now such essential achievements, both chronic and occasional, cannot be included in any scheme of organization for they usually lie outside the purview of the investigator himself or depend on situations over which he has no control. Discovery and invention are in this sense fortuitous or accidental and also involve a time factor which is equally unpredictable and unorganizable. The investigator, if you will pardon my emphatic language, can only do his damnedest and hope that the new truth will deign to ascend from the subconscious or descend from the lay of the gods. After long and tedious observation or experiment and many dis-

appointments he may or he may not find the discovery or invention flashing suddenly and more or less completely into consciousness or emerging from some happy constellation of events. The plant-physiologist Sachs once told me that his best ideas suddenly entered his mind in the morning while he was lacing his shoes or brushing his teeth. I have noticed in my own case that the few unimportant ideas that strike me as unlike those which ordinarily infest my waking consciousness emerge suddenly while I am passing a certain vacant lot on my morning trip to my laboratory. Not improbably my single cup of breakfast coffee may be a stimulus so timed that the reaction coincides with the vacant lot. I hasten to confess, however, that the outline of this paper was not picked up in a vacant lot, as its miscellaneous contents might lead you to suppose, but came to me, probably after prolonged subconscious incubation, while I was wondering how much coal I could save by using as an *Ersatz* the literature received during the past three years from that noble superorganization of superorganizers, the National Research Council.

4. I have dwelt on the amateurs because they seem to me to form another insuperable obstacle to the organization of research, at least in the biological field, where they constitute a very large and important "bloc" of investigators. While one might be pardoned for supposing that some of the house-broken or domesticated investigators, who indulge in what is called "institutional" or "industrial" research might be organized after a fashion, it would be unpardonable to suppose that the wild, untamable amateurs would ever submit to such an indignity. These seem to be described as "solitary workers" in some of the literature I have

received — why, I cannot say. The amateur, as the word implies, is a lover, and all the world loves a lover, no matter how wild, or just because he is wild. Certainly the many members of our numerous natural history, ornithological, entomological, malacological, botanical, and mycological clubs, who hold monthly meetings and contribute modestly but effectively to the sum of our knowledge, regard themselves as anything but "solitary" workers. That designation would seem to be more applicable to some of the professionals in our universities and research institutions.

Of course, the organizer who has been stung by the efficiency bug is troubled by all this diffuse and elusive activity and counters with the assertion that organization would save duplication of effort and direct it to problems of fundamental importance. This takes for granted a knowledge of the fundamental problems on the part of the organizer and a most enviable intuition of the means adapted to their solution, or, at any rate, seems to imply that working on fundamental problems means *eo ipso* making important discoveries and inventions. The contention that we must avoid duplication of effort must have had its origin in a machine shop or a canning plant, for it certainly never originated in the brain of any investigator worthy of the name. That the establishment of the simplest item of our knowledge not only requires duplication, but reduplication and re-reduplication of effort is too obvious to require discussion, as is also the fact that we always regard the agreement in the results of two or more investigators working independently as presumptive evidence of truth. I would similarly pass over the further implication in the arguments of the organizers, that the only value of an investigator's work lies in the scientific

data and conclusions which it contains, and that we are not concerned with its unconscious revelations of habits of thought, personality, etc. The perusal of the works of the great amateur entomologists, Réaumur and Fabre, might be recommended for those whose minds are in such a ligneous, arenaceous, or argillaceous condition.

The suggestion that scientific research may be advantageously organized naturally leads one to consider those other great human activities, religion and art, which are also bound up with powerful instincts, emotions, and interests. Certainly religion, especially in the form of dogma and ritual, has been so superbly organized *semper ubique et omnibus*, since it first arose in the totemism, taboo, and magic of our savage ancestors, that it would seem to constitute a wonderful field for the study of both the blessings and curses of organization. It is, in fact, a field in which organization could be readily introduced and maintained owing to the proneness of so many human beings to suggestibility, credulity, the gregarious instinct, the instincts of self-abasement and fear, and the sentiments of awe and reverence — all of which, be it noted, are singularly feeble or defective in the investigator. The same conclusion would seem to follow from the very different view of some of the Freudians, who state that all religions are permeated by a subterranean feeling of guilt and that "this absolutely unfailing presence of the feeling of guilt shows us that the whole structure of religion is erected on a foundation of repression of instinct."[12] That the perfection of organization so character-

[12] Cf. O. Rank and H. Sachs, *The Significance of Psycho-analysis for the Mental Sciences*, tr. by C. R. Payne, Nervous and Mental Disease Monograph Series, No. 23 (New York, 1916), p. 71.

istic of religion may have been beneficent in other times may be admitted, but the more nearly perfect an organization, the less it is able to adapt itself to changing conditions, and the World War has disclosed to all thinking men the same kind of hopeless, resourceless overspecialization in our ecclesiastical organizations as that with which the biologist is so familiar in archaic, moribund, and actually extinct species. At the present time the Church seems to be about as well adapted to piloting the great forces which are impelling society as a two-toed sloth to piloting an airplane or a manatee the Twentieth Century Limited. Like the Edentate and the Sirenian the Church exhibits such feebleness of volition and muscular tonus and such a low ebb of creative energy that one is inclined to find a modicum of truth in the aphorism which H. G. Wells saw posted by the bolsheviki on one of the houses in Moscow: "Religion is the opium of the people."

What a different picture is presented by that other great field of human activity, in which the instinct of workmanship and the creative imagination attain their finest and most unrestrained expression — the field of art! Its very life seems to depend on freedom from all imposed organization. Hence its plasticity and adaptability in all ages and places, its resilience and prompt resurgence after periods of conventionalization, or overspecialization. Unlike the religious person who seems always to be mistrusting his instincts, or the scientific investigator who is so sophisticated that he ignores them, the artist takes them to his bosom, so to speak, and in all his works tries to persuade the rest of the world to do the same. He thus becomes the ally of creative Nature herself and while himself capable of such control and restraint as

are demanded in the harmonious execution of his work, quickly resents the slightest suggestion of restraint or control from the outside. This is so well known that one would find it more entertaining than informing to hear the comments of a lot of painters, sculptors, composers, poets, novelists and actors — and especially of a lot of actresses or prime donne — if some National Art Council had the temerity to suggest that their work could be greatly improved by organization.

The history of science and philosophy is not without significance in connection with the attempts of modern organizers. It is well known that both, after their twin-birth and brilliant childhood among the Greeks, lived through a kind of stupid Babylonian captivity as hand-maidens to the Mediæval Church, which had been so successful in organizing itself that it naturally tried to organize everything else. But science turned out to be such an obstreperous and incorrigible tomboy that she long since regained her freedom, and philosophy, though she had been treated with more consideration, and may still occasionally flirt, no longer, outside of our Jesuit colleges at least, sits down to spoon with theology as she did in the days of St. Thomas of Aquin.

Times have changed so greatly that at present we even have eminent amateurs, like the Rev. Erich Wasmann, S.J., who vie with Haeckel in the boldness of their evolutionary speculations. Scientific research is no longer concerned with the Church but with the two great forces which are contend for the mastery of the modern world, labor and capital. The present plight of the Russian investigators shows us, perhaps, what we may expect when certain communistic ideals of labor are put into practice, and Veblen's account of

the evolution by atrophy of the creative artisan of former centuries into the modern factory operative, whose life has been reduced by capital, machinery, and efficiency experts to one long hideous routine in some overspecialized task, shows us, perhaps, what we may expect when nothing but money talks.

Even if the investigator could hold aloof and adopt a policy of watchful waiting, till the world is controlled by either labor or capital or, as seems more probable, by some compromise between them, he would still be in an unfortunate position. Since both labor and capital are primarily concerned with production, we should expect both to center their interests on applied research, or invention, and to neglect research which is fundamentally concerned with discovery. This would be unfortunate, because the two kinds of research can be most fruitful only in symbiosis, for the neglect of discovery must lead to impoverishment of the theoretical resources of the inventor, and purely theoretical research strongly tends to become socially ineffective. We have as yet, I believe, no concise information in regard to labor's attitude to so-called pure research. The attitude of the capitalist, or business man seems to be much more definite. His activities, like those of the investigator, are bound up with certain powerful, highly conditioned instincts, emotions and interests, some of which have been elucidated by Taussig.[13] He believes that the business man is driven mainly by the acquisition instinct, centered of course on pecuniary profits, the instinct of domination or predation, the instinct of emulation, in the special form of social emulation, and the instinct of devotion or altruism. Undoubtedly

[13] *Inventors and Money-Makers* (New York: Macmillan, 1915).

we must recognize also the importance of the instinct of workmanship as a powerful drive in many eminent business men, but both it and the instinct of devotion are, of course, apt to be directed to practical matters or to those which yield immediate returns, such as philanthropy, charity, medicine, etc. Apart from certain notable exceptions, business men may, therefore, be expected to favor invention and to take little interest in discovery, except when it relates to natural resources capable of exploitation.

These considerations lead me to the opinion that so long as our present society endures adequate financial and other support for research in its most comprehensive form will be forthcoming only after the general community has thoroughly grasped the fact that of the four great fields of human endeavor, science, art, religion, and philosophy, science is of the most overwhelming social value in the sense that the welfare of every individual, physically, mentally, and morally, absolutely depends on its developments, or in other words, on scientific research. To saturate the general public with this conviction is a formidable task and one that can be accomplished only by a slow process of education.

There is also another aspect of the subject which I can best make clear by returning to that form of organization which we observe inhering in individual animals and plants and in the societies of the former. Occasionally we find such organisms so highly integrated, differentiated, or specialized as seriously to impair their powers of adaptation. When such a condition is reached, the organism either persists without phylogenetic change, if its environment remains stable, or soon becomes extinct, if its environment changes. Most organisms, however, retain a lot of relatively

unorganized or more or less generalized structures and functions as reserves for prospective adjustments to the changing environment. Our own bodies still contain many such primitive elements, like the white blood corpuscles, the undifferentiated connective tissue, dermal and glandular cells; and in larval insects we find even undifferentiated nerve cells. And we all carry with us in our subconscious a great reservoir of very primtive instincts and tendencies, many of which are as archaic as those of our Palæolithic and anthropoid ancestors. This whole relatively undifferentiated and imperfectly organized equipment must be of the greatest value as a source of future adaptations.

We are also beginning to see that as civilization progresses it is necessary to maintain a certain number of our activities in a primitive, unorganized condition and for their exercise to set aside hours of leisure and relaxation, vacations and holidays, so that we can escape from the organized routine of our existence. And as the earth becomes more densely covered with its human populations, it becomes increasingly necessary to retain portions of it in a wild state, that is, free from the organizing mania of man, as national and city parks or reservations to which we can escape during our holidays from the administrators, organizers, and efficiency experts and everything they stand for and return to a Nature that really understands the business of organization. Why may we not regard scientific research, artistic creation, religious contemplation, and philosophic speculation as the corresponding reservations of the mind, great world parks to which man must resort to escape from the deadening, overspecializing routine of his habits, mores, and occupations and enjoy veritable creative holidays of the spirit?

These world parks are in my opinion the best substitute we are ever likely to have for the old theological Heaven, and they have the great advantage that some of us are privileged to return from them with discoveries and inventions to lighten the mental and physical burdens of those whose inclinations or limitations leave them embedded in routine. This is the meaning of that stanza in the witch's song of Faust:

> The lofty skill
> Of Science, still
> From all men deeply hidden!
> Who takes no thought,
> To him 'tis brought,
> 'Tis given unsought, unbidden!

Like other members of society, the scientist, artist, and philosopher must always devote considerable time and energy to routine occupations, for their lives, with very rare exceptions, are not completely absorbed in research, speculation, and creative activity. They might therefore be expected to react rather unpleasantly to any suggestion of meddling with those occupations in which they feel that they can express their personalities with the greatest freedom and the greatest satisfaction to themselves if not to others. It seems to me that it can only be due to the modesty or indifference of scientific investigators that they have failed to voice their opinions of the organizers. The only utterances I have seen are an admirable paper by Professor Sumner [14] and in another field, that of social theory, a few paragraphs by G. D. H. Cole,[15] which are partly quoted at the beginning of this paper.

[14] "Some Perils Which Confront Us as Scientists," *Scientific Monthly*, March 1919, pp. 258–274.

[15] *Social Theory* (New York: Frederick A. Stokes Co., 1920), p. 185.

VI

THE DRY-ROT OF OUR ACADEMIC BIOLOGY [1]

You beat them and they give out dust like meal sacks. But who could guess that their dust came from corn, and the golden wonder of the summer fields? — NIETZSCHE, *Thus Spake Zarathustra*

In all institutions which are not ventilated by a keen draught of public criticism, an innocent corruption grows up like a toadstool (for example, in learned corporations and senates). — NIETZSCHE, *Human All Too Human*

In the modern world the celibacy of the medieval learned class has been replaced by a celibacy of the intellect which is divorced from the concrete contemplation of the complete facts. — A. N. WHITEHEAD, *Science and the Modern World*

Truly, as William James once exclaimed to me, apropos of the policy of certain philosophers, "the natural enemy of any subject is the professor thereof!" It is clear that if these tendencies are allowed to prevail, every subject must in the course of time become unteachable, and not worth teaching. Thus educational systems become the chief enemies of education, and seats of learning the chief obstacles to the growth of knowledge, while in an otherwise stagnant and decadent society these tendencies sooner or later get the upper hand and utterly corrupt the social memory. The power of the professor is revealed not so much by the things he teaches, as by the things he fails or refuses to teach. — F. C. S. SCHILLER, *Tantalus, or the Future of Man*

OUR SOCIETY requires its retiring president to close the annual meeting with a discourse or sermon — a task which has become increasingly difficult, for every year the program of the morning and afternoon sessions becomes more abstruse and therefore makes greater demands on our

[1] Address of the president of the American Society of Naturalists, Boston, December 29, 1922, published in *Science*, January 19, 1923, and in *Foibles of Insects and Men* (New York: A. A. Knopf, 1928); included here by permission of The Science Press and A. A. Knopf.

attention and the lingering memories of past presidential rhetoric invite to more odious comparisons. To me the task was the more arduous, because I had been busy for many years in remote fields of entomology in which few of you are interested, and because it fell to me at an inopportune moment, while I was in the very act of laying — if you will pardon a French expression — a volume of some 1,100 pages on ants. This racking oviposition leaves me reduced to a mere blob of *corpora lutea* and so feeble that I can only crawl, in search of a text for my sermon, to the next Encyclopædia Britannica article, which is not "ant-eater," but "Antæus." You will recall Antæus, that mythical F_1 generation hybrid between Poseidon, the Sea, and Gaia, the Earth. His hybrid vigor was so great, we are told, that he not only grew to gigantic stature, but insisted on wrestling with every stranger that happened to pass through his Libyan domain. He was always invincible in these encounters because his strength waxed with each successive contact with his mother Earth. When not engaged in wrestling he was building a monument to his father with the skulls of the vanquished. One day Hercules came along and, knowing the secret of the giant's strength, raised him aloft and strangled him in the air.

We may, perhaps, interpret this exploit of the sun-god Hercules as a mythical expression of the fact that no terrestrial substance can permanently resist evaporation or volatilization by heat, but the accepted and, I believe, more manifest meaning of the myth is that even an agile and vigorous mortal had best keep his feet on the concrete if he wishes to avoid death at the hands of the Hercules of abstraction. That the myth is of rather late origin would

seem to be indicated both by this somewhat sophisticated interpretation and by the fact that the slaying of Antæus was not one of the twelve great labors of Hercules, but one of his Parerga, or deeds done by the way. The athletic demi-god, while sprinting across the Sahara to get the golden apples of the Hesperides, merely stopped for a few minutes to finish Antæus. One might conjecture that the myth had been invented by some malicious Athenian potter or weaver, who, happening to live next door to the Academy, had often been annoyed by the "hot air" emanating from that institution, were it not that an Antæus-Hercules wrestling bout is known to have been a brilliant scene in one of the lost dramas of Phrynichus, written about 500 B.C.[2] Nevertheless, the myth remains to this day as one of the most beautiful expressions of the practical man's attitude toward those who place too much confidence in their more abstract intellectual operations.

After securing this text there was difficulty with the title of my sermon. I could not decide whether to call it the "tommy-rot" or the "dry-rot" of our academic biology. I finally chose the latter, because some of our activities so closely resemble the inroads of the fungus *Merulius lacrymans*[3] in old timber, and because it might be amusing to

[2] One may also conjecture that the story of Antæus is a very ancient but much distorted vegetation myth. It certainly resembles the myths of the Phrygian Lityerses and the Lydian Syleus. Both of these vegetation gods compelled strangers to compete with them, the one in the corn-field, the other in the vineyard, and both habitually slew their competitors and were in turn slain by the passing Hercules. See J. G. Frazer, *The Golden Bough*, abridged ed. (New York: Macmillan, 1922), pp. 425, 442.

[3] The wood-rots, and especially *Merulius lacrymans*, are characterized as follows by R. T. and F. W. Rolfe (*The Romance of the Fungus World*, Philadelphia: Lippincott, 1926, p. 117): "By means of the rhizomorphs, these fungi are enabled to climb over brick, stone, or metal, in their apparent

find that the conscientious cataloguers of the Widener Library had included my effusion under cryptogamic botany or phytopathology. Imagine the hilarity of some young foot-ball player in the year of our Lord 1952, condemned to bone up for a final exam, and happening on a reprint of this paper reposing unashamed between such monuments of cryptogamic erudition as the seventy-four folio volumes of Professor Farlow's *Toadstools of God's Footstool* and the twenty-seven quarto volumes of Professor Thaxter's *Laboul-*

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search for more distant woodwork. Their development, for which water is necessary, sometimes proceeds at an extraordinarily rapid rate. Some, e.g. *Coniphora cerebella*, cannot flourish except in really damp wood, and can thus be readily destroyed, even when they have gained a footing, by drying the wood and ensuring a good ventilation for the future. Others, although requiring moisture for the germination of their spores, can, when established, produce the necessary water themselves. This is the case with *Merulius lacrymans*, the fungus responsible for 'dry-rot.' This requires air for respiration, during which process it takes up oxygen and gives out water, which may thus be produced to the extent of half as much by weight as the original wood. The presence of these drops of water on the hyphæ have been responsible for its name of *lacrymans* — 'weeping.' This faculty, in conjunction with its frequency of occurrence, and its habit of growth, renders *Merulius lacrymans* probably the most dangerous of any timber-rot, for it can attack the driest wood, and cannot be eradicated by drying or ventilation, unless heat is applied. It affects not only soft woods but also hard woods, including teak, oak, and mahogany, which in the space of a few years, or under exceptional circumstances in a much shorter time, may be practically destroyed. The woody tissue permeated by mycelium is left as a spongy mass of brownish material, which absorbs water so as largely to retain its original dimensions while wet. The drying-up of the material produces, however, the well-known 'dry-rot' effect, showing a multiplicity of cracks often disposed more or less at right angles to each other. The power of 'locomotion' of this fungus is really remarkable, for it has been known to travel for yards along thin tubes containing bell-wire; it can climb up a wall from one floor of a building to the next, and it can even penetrate brickwork through the mortar, involving in a common disintegration the walls no less than the woodwork. *Merulius lacrymans* appears to be almost always found in buildings, and very rarely in woods, for which reason it is known to the Germans as 'Hausschwamm,' or house-fungus."

beniales of the Universe — like a naughty tick pressed between the hide of some royal Siamese she-elephant and that of her suckling daughter![4]

Text and title having been selected, autopsychoanalysis, which, like prayer, is now one of my favorite diversions, revealed the fact that I was suffering from an acute, repressed desire to commit sabotage on our academic biology by hurling a monkey-wrench into its smug machinery. Since, according to the Freudians, such desires simply must be satisfied, and since I may never have another opportunity to hit so many of the wheels with one shot, I can see no reason why I should not obtain all the catharsis to which psychopathology entitles me. My mental condition is, no doubt, partly due to the disappointing spectacle of our accomplishments as more or less decayed campus biologists in increasing the number, enthusiasm and enterprise of our young naturalists. I estimate that at least 25 per cent of all students graduating from our colleges have had the equivalent of an elementary course in zoology or botany.[5] There must be many thousands of these young men and women in the country and yet, in a prosperous population of 110,000,000, the number with a vital and abiding interest in biological inquiry, even as an avocation, is extremely small. And in our universities, apart from the students preparing to enter medicine, the number indulging in advanced and graduate courses in the science would probably shrink to zero if we failed to provide fellowships or to hold out to them at the

[4] I have since learned that this article was actually catalogued and filed among the literature on fungi in one of our college libraries.

[5] Cf. the very temperate article by Professor H. H. Nininger, "Zoology and the College Curriculum," *Scientific Monthly*, XVI, 66–72 (1923), an article which I did not see until after the delivery of my address.

end of a long pole that enhaloed bundle of hay, the doctor's degree.

Is this situation due to the moronic ignorance or the satanic machinations of our trustees, presidents and deans? I take down Professor Cattell's illuminating monographs on the taxonomy and behavior of this fauna, but cannot find that it is to blame. Is it the fault of the students? Obviously not, for no country produces a greater and more sweetly docile mass of pedagogical cannon-fodder. It would seem, therefore, that the teaching of biology should not be entrusted to those whom Bismarck called the damned professors, or that there is something wrong with us who try to teach the science, or with the environment in which we carry on the business. I cannot avoid the impression that the problem involves, in varying degrees, all three of these factors. Of course, their adequate discussion would be extremely wearisome. I can only pull out little mycelial tufts of *Merulius lacrymans* here and there and submit them to your inspection as evidence of the dry-rot which seems steadily to be invading the underpinning of biology, at least in some of our eastern universities. If you can bear with me, after a day of strenuous attention to far worthier utterances, I shall first consider very briefly some of the disabilities, both material and personal, under which we seem to be laboring, and in conclusion suggest what I believe might be an ameliorative if not a remedial plan of action.

The hampering effects of the material and environmental conditions under which we strive to inspire the young to become life-long naturalists deserve more attention than they have received. Any one of us who endeavors to grasp with

his poor intellect, enfeebled by years of gyration in the academic mill, the stupendous and confusing accumulation of facts, not to mention the assumptions, fictions, hypotheses, theories, and dogmas that make up present-day biology, must be staggered by the difficulty of selecting the most appetizing, concentrated, and nourishing food for the student just entering the academic cafeteria. Perhaps no other collegiate department is expected to deal with such a vast and heterogeneous wealth of potential pedagogical pabulum. And the difficulty is greatly increased by the fact that one and all of us are highly specialized cooks, who delight in feeding the young on the dishes we ourselves like or that mother used to make and incidentally in showing our fellow cooks what delicious messes we can prepare. The student's metabolism may require plain gruel and toast, but we often insist on filling him up with so many elaborate pastries and salads that we ruin his digestion and, what is a thousand times worse, his appetite. Please bear in mind that I am trying to discuss the very practical business of teaching, not research. I am, of course, a ritualistic, high-church, port-and-sherry-loving Episcopalian in research, but only a poor, Peruna-soaked Methodist when it comes to teaching. I would go to such absurd lengths in helping research that I would even provide a room in the very modest institution to which I belong for any young man who might wish to spend the next ten years of his life investigating, say, the nucleolus of the fourth cell from the end of the last caudal cartilage of the embryo chipmunk, and if his work became very absorbing and his digestion impaired, I should be willing to feed him through a tube in the wall till his head swelled to the size of the room and he believed that he had

become the nucleolus of Betelguese, but I should not permit him to see, much less converse, with freshmen. Such a pearl should not be cast before swine.

We might regard it as a great handicap that we academic biologists, unlike our native woodchucks and muskrats, are compelled to be most active pedagogically during the annual glacial period, but our superior intelligence enables us to cope with that situation. Every autumn we lay in a few cans of soused dog-fish and pickled sea-cucumbers, coop up some guinea-pigs, earth-worms, cockroaches, and fruit-flies, throw in a bag of beans and several bales of hay for the botanists — and we are prepared for the worse. We can now proceed to disentangle and unreel the infinite and ineffable complexity of organic reality. We have more than enough for the purpose, for were we not all taught in our childhood by some old maid with ringlets that any little flower, or any little bug, for that matter, plucked from the crannied wall and held in the hand, is sufficient? When the neophyte becomes nauseated with the mess we have provided we can encourage him and incidentally heighten our own prestige by telling him that he is learning to forecast and control the behavior of organic nature, that he may shortly be able to make real live homunculi and regulate their mating habits, and all the pishpash with which, since the Neolithic Age, other priests and other wizards have heartened their constituencies.

More important than the drawbacks I have hinted at are certain types of personality engaged in the business of teaching biology. Since the inquiring scientist insists on poking his nose into every fold of reality, and since biology professors constitute a part, and, in their own estimation at least, an important part of reality, we might expect them not only

joyfully to investigate the behavior of their colleagues — they do this already — but also to submit themselves to investigation, with at least a show of good grace. What startling results we might hope to obtain from a thoroughgoing application of the Freudian and Adlerian analyses and the intelligence tests! But even if we concede that the damned professor is an extraordinary being because he has sufficient inertia to specialize for a life-time in a particular department of learning, we must admit that he will grow old like the most ordinary individual of his species. He will gradually take on most or all of the stigmata of gerontic involution, which Dr. G. Stanley Hall has enumerated. At forty, if not sooner, his sense-organs, musculature, endocrines, emotions, and memory will begin to atrophy and his intellectual processes will become more and more stereotyped, dogmatic, and abstract. From a young Antæus continually gaining fresh strength from each successive contact with concrete reality he will become a creature increasingly infatuated with generalizations, relationships, and hypothetical explanations, especially if they are of his own confection, and he will eventually drift into a stage in which words, formulæ, and imaginary entities become the very breath of his nostrils. He has been borne aloft to be slowly asphyxiated in the tenuous atmosphere of the unreal. There are, of course, all degrees of the process, and it is so gradual that it may completely escape even a professor. One rather mature student, who had spent four years in a divinity school, recently told me that, having outgrown theology, he had entered the course of one of our eminent geneticists, a man capable of twisting one's head off were one to insinuate that he had ever released his feet from the concrete. A few weeks later the stu-

dent quietly dropped the course and when asked the reason replied that the professor's mental processes were so similar to those of his decrepit divinity teachers when they held forth on predestination, salvation through grace, infant damnation, and the like, that he had decided not to add a fifth year to his theological training.

Unfortunately we have no intelligence tests for individuals with a mentality of more than eighteen years, and biologists are supposed to be older, though some of them somehow manage to harmonize a physical age of forty to sixty with a mentality of eight to fourteen. These, however, if really human and endowed with a decorative personality, seem to make the best teachers, probably because they enter most readily into mental rapport with the freshmen and sophomores. It is not from such professors that the Merulius spores proliferate most profusely, but from those who have a physical age of forty to sixty and a mental age of eighty to one hundred and five.

I do not wish to be misunderstood on this matter of aging. Those of us whom the gods have not sufficiently loved to remove early in life all develop what might be called the normal inferiority complex of senescence, but we rationalize and compensate or even overcompensate for it. This is apparent in all the discussions of the subject from the remarks of the aged Cephalus in the prologue of Plato's *Republic* and Cicero's *De Senectute* to the very recent essay of the still delightfully youthful Professor Jennings *On the Advantages of Growing Old*. La Rochefoucauld put the matter concisely when he said that "old men are fond of giving good advice in order to console themselves for being no longer able to serve as bad examples." As youngsters we are

all filled with a spirit of adventure and long to dominate reality; later, after we have worn down our eye-teeth on its resistant carapace, we try to compromise with it by cajolery, and when this, too, fails, we forsake it and create a reality of our own, a realm of ideas, Platonic, esoteric, inviolable, eternal, in which we can still exercise the meager remnants of our will to power. This type of senescent compensation is most beautifully displayed in the sheltered environment of our universities, and I would not underestimate its enormous value to science and therefore to the race. It is clearly exhibited by old or prematurely old taxonomists, morphologists, and geneticists, who derive from static fictions like species, unit characters, genes, etc., a certain feeling of potency, of having their fingers on the very vitals of organic reality. Many of our most revered biological hypotheses are the work of senescents who have been sufficiently industrious and ingenious to make their subconscious compensatory strivings tally with very considerable bodies of facts. It would be interesting to ascertain the precise age, conditions of the sense-organs, endocrines, etc., of men like Darwin, Spencer, Galton, Weismann, Bruecke, Naegeli, Haeckel, Jaeger, Altmann, Wiesner, Haacke, Brooks, Verworn, De Vries, Hatschek, and Johannsen when they first began to operate with pangens, biophors, and similar ultra-microscopic flora. We might also need the cephalic index, since certain racial tendencies may be involved. This is suggested by the fact that the French and Italian biologists have rarely shown the slightest interest in the construction of such entities. Are these biologists deficient in imagination or analytical power? Hardly. Or must we assume that the French and Italians, after having produced so many of the

great scholastics, have lost confidence in their methods of dealing with the phenomenal world?

Undoubtedly the best culture medium for the academic dry-rot fungus consists of about equal parts of narrow, unsympathetic specialization and normal or precocious senile abstraction; and as this medium is always present in many personalities that find their optimum environment in our universities, the outlook is depressing. A friend who has long been studying our institutions of learning maintains that our only salvation lies in discharging all our faculties and burning or thoroughly disinfecting all the buildings every twenty-five years. I am somewhat less pessimistic, for although I have seen very little improvement in pedagogical method in our biological departments during the past thirty-five years, the stress they have laid on research has preserved them from the hopeless mummification that has overtaken some of the other departments.

It seems to me that there are two periods when the young biologist is most susceptible to lethal infection by the Merulius spores that are continually being thrown off by his professors. One is his freshman year, when he should be stimulated to develop an enthusiastic, receptive attitude, the other his graduate year or years, when he may be expected to adopt an independent, adventurous, and creative attitude toward his science. Of course, the treatment of advanced students is easy for any professor who will follow the excellent example of the late Professor Roland of Johns Hopkins. The story is told that he was once presented with a list of rules for teaching graduate students and that he crossed out all the items and wrote beneath: "Neglect them!" Despite this very convenient precept, many of us coddle our graduate

students till the more impressionable of them develop the most sodden types of the father-complex. Some of us even wear out a layer of cortical neurones annually, correcting their spelling and syntax. One fussy old guru of my acquaintance has destroyed both of his hemispheres, his corpus callosum, and a large part of his basal ganglia hunting stray commas, semicolons, dashes, parentheses, and other vermin in doctor's dissertations.

Not only do many of us wear out our most valuable tissues converting the graduate students into mere vehicles of our own interests, prepossessions, and specialties, but nearly all of us fail to excite in them that spirit of adventure which has in the past yielded such remarkable results in the development of our science. The finest example of this lack of vision is seen in the stolid indifference, especially in our eastern universities, to exploration and research in the remoter portions of our own country, in foreign lands, and especially in the tropics. We have in the Philippines and at our very doors in the West Indies, Mexico, Central and South America the most marvelous faunas and floras in the world, but we still persuade our traveling fellows to cut more sections in the laboratories of Professor Rindskopf of Berlin or Professor Himmelschwanz of Leipzig, because thirty or forty years ago we were sent to the same *bemooste Häupter*. There was then a certain justification for this procedure, because we at least picked up much valuable information from our fellow students in the *Bierstube*. But what shall we say to such dry-rot exhibitions as the following? A few years ago I was asked to secure a young botanist to accompany a biological expedition to the little-known Solomon Islands and therefore begged one of our eminent

exsiccati to aid me in the quest. To my amazement he actually asked me whether I did not know that New England was covered with a luxuriant and almost unknown flora and did not regard it as a crime to dissuade a young botanist from devoting his life to pressing the plants of Cape Cod! And yet the theory which has revolutionized all our thinking was brought to us from the tropics by two naturalist explorers, and for a century those who have presided over higher education in Great Britain, France, Germany, and the Scandinavian countries have seized every opportunity to send their young biologists to the tropics. I refrain from wearying you with the long list of gifted European naturalists who, just before the war and throughout the tropics of both hemispheres, were increasing our biological knowledge by leaps and bounds. The neglect of our splendid opportunities has, in fact, become such a scandal that it is known even to our august band of Delphic hierodules [6] in crinolines, the National Research Council.

When we leave the advanced student and turn to the beginner, the picture is even more depressing. To us gerontic schoolmarms in trousers, who have flown from reality and have slowly succumbed to autistic thinking, with defective eye-sight, doughty musculature, brittle ossifications, demoralized intestines, decayed autonomic nervous systems, and atrophied interstitials, there comes every year a small army of freshmen — very properly so called — in the late teens and early twenties, burning for impact with reality, with exquisite sense-organs, superb bones, muscles, and alimentary

[6] The definition of "hierodule" in the Century Dictionary is followed by the remark: "Large numbers of such slaves were attached to some foundations, and were either employed about the sanctuary or let out for hire for the profit of the god."

tracts, mirific endocrine and autonomic apparatus and a mentality of nine to fourteen years, or thereabouts — and what do we give them? Perhaps we give them what they deserve for coming to us, but it might be more charitable to discuss what we do not give them. What portion of the science of life, that most concrete and most entrancing of all the sciences, ought we to administer to this suckling host of postadolescents? I answer: they should be fed during the first year on the simple oat-meal pap of ecology, but I hasten to declare that I do not mean the "ecology" of the zoologists, and especially of the botanists, of what Mencken calls the silo and saleratus belt of our great republic. For the sake of defining my meaning I shall have to make another tedious digression.

If, as some one has said, mathematics is the science that gives a single name to a great many different things, biology is certainly the science that gives a great many names to the same thing. This is an old story to the taxonomist, who, if he be worth his salt, will not only confer as many names as possible on every animal and plant, and change those of the commonest species every six months, in order to apprise other biologists that he is on the job, but he will also consign as many as possible of the other fellow's names — especially if he dislikes the other fellow — to the synonymy. I admire Haeckel, but I dislike his term "ecology" and have repeatedly pointed out that it belongs in the synonymy with a number of other terms, ranging in order of priority as follows: "natural history" (eighteenth and nineteenth centuries), "ethology" (Isidore Geoffroy St. Hilaire, 1859), "ecology" as "Relationsphysiologie" (Haeckel, 1866, 1869), "Biologie" in the restricted German sense (later nineteenth century to present),

"bionomics" (E. Ray Lankester, 1889), "behavior," "comportement," "Gebaren" (past three decades). In this country the inept Haeckelian term, largely as a result of the aforementioned silo and saleratus botanists and their zoological camp-followers, has won the day and my adrenals are now too weak to offer further resistance.

Huxley, writing in 1879, apparently distinguished three ontogenetic and phylogenetic stages in the development of biology. He says:

> Every country boy possesses more or less information respecting the plants and animals which come under his notice, in the stage of common knowledge; a good many persons have acquired more or less of that accurate, but necessarily incomplete and unmethodized knowledge, which is understood by Natural History; while a few have reached the purely scientific stage, and as Zoologists and Botanists, strive towards the perfection of Biology as a branch of Physical Science.
>
> Historically, common knowledge is represented by the allusions to animals and plants in ancient literature; while Natural History, more or less grading into Biology, meets us in the works of Aristotle, and his continuators in the Middle Ages, Rondeletius, Aldrovandus and their contemporaries and successors. But the conscious attempt to construct a complete science of Biology hardly dates further back than Treviranus and Lamarck, at the beginning of this century, while it has received its strongest impulse, in our own day, from Darwin.[7]

This view of the matter is no longer adequate, quite apart from the fact that we are now entering on a fourth stage, a kind of metabiology, embracing biochemistry. The first of Huxley's stages, that of "common knowledge," should have been differently presented, in order to emphasize the prac-

[7] *The Crayfish: An Introduction to the Study of Zoology* (London: C. Kegan Paul & Co., 1880), p. 4.

tical, or economic source of the science in the activities and lore of the hunter, trapper, woodsman, herdsman, fisherman, husbandman, gardener, herbist, midwife, medicineman, etc. His second stage, that of "natural history," seems also to be presented in an inadequate, if not misleading manner, probably because he was primarily a morphologist and somewhat dazzled by the fresh effulgence of the Darwinian theory of evolution, so that he seems to treat natural history not only as a transitional but also as a transitory phase in the development of biological science. History shows that throughout the centuries, from Aristotle and Pliny to the present day, natural history constitutes the perennial root-stock or stolon of biological science and that it retains this character because it satisfies some of our most fundamental and vital interests in organisms as living individuals more or less like ourselves. From time to time the stolon has produced special disciplines which have grown into great, flourishing complexes, and it has itself changed its name from time to time as the investigators of different periods have been impressed by different aspects of its fundamental tendencies. Aristotle wrote of the "histories" of animals, the naturalists of more recent centuries spoke of their "habits"; we have become more articulate and speak of their "behavior." Even a superficial acquaintance with the voluminous writings on natural history from those of the Stagirite to those of Gessner, Réaumur, and Buffon and the naturalists of the first half of the nineteenth century, shows that for obvious psychological reasons human interest in organisms has always centered in their activities or what we now call their reactions to stimuli, their adjustment or adaptations to their environment and to one another. By the latter part of that pedantic century, the

eighteenth, such great reserves of observation and experimentation had accumulated in the stolon that it began to bud. Taxonomy, morphology, paleontology, physiology began to shoot up, branch, and differentiate, becoming independent specialties, developing their own methods, fictions, and hypotheses. In the middle of the nineteenth century, after the great voyages of exploration, the bud chorology, or geographical distribution, appeared; and about the same time I. G. St. Hilaire and Haeckel, wishing to emphasize the fundamental importance of adaptation, but mistaking the stolon for a bud, named it "ethology" or "ecology." More recently another dear little bud, genetics, has come off, so promising, so self-conscious, but, alas, so constricted at the base. And future centuries will no doubt witness a further gemmation of biological disciplines from the same old natural history stolon.

This is, of course, an extremely imperfect and summary sketch of the development of biological sciences, but it emphasizes the primitive, central, and dynamic source of our interest in organisms. Obviously we can offer no criticism of those who prefer to call natural history or ecology "general" or "external physiology." Burdon Sanderson in 1893 presented the matter very concisely from this point of view in the following passage:

> Now the first thing that strikes us in beginning to think about the activities of an organism is that they are naturally distinguishable into two kinds, according as we consider the action of the whole organism in its relation to the external world or to other organisms, or the action of the parts or organs in their relation to each other. The distinction to which we are thus led between the *internal* and *external* relations of plants and animals has of course always existed, but has only lately come into

such prominence that it divides biologists more or less completely into two camps — on the one hand those who make it their aim to investigate the actions of the organism and its parts by the accepted methods of physics and chemistry, carrying this investigation as far as the conditions under which each process manifests itself will permit; on the other, those who interest themselves rather in considering the place which each organism occupies, and the part which it plays in the economy of nature. It is apparent that the two lines of inquiry, although they equally relate to what the organism *does*, rather than to what it *is*, and therefore both have equal right to be included in the one great science of life, or biology, yet lead in directions which are scarcely even parallel. So marked, indeed, is the distinction, that Professor Haeckel some twenty years ago proposed to separate the study of organisms with reference to their place in nature under the designation of "œcology," defining it as comprising the relation of the animal to its organic as well as to its inorganic environment, particularly its friendly or hostile relations to those animals or plants with which it comes into direct contact. Whether with the œcologist we regard the organism in relation to the world, or with the physiologist as a wonderful complex of vital energies, the two branches have this in common, that both fix their attention, not on stuffed animals, butterflies in cases, or even microscopical sections of the animal or plant body — all of which relate to the framework of life — but on life itself.[8]

The stolonic relationship of natural history, or ecology, to the other biological disciplines is of great theoretical and practical significance. Nearly all the important biological problems, especially of a physiological or morphological character, have arisen in the course of simple investigation in natural history and many of the more difficult of them have

[8] Presidential Address, *Report of the Sixty-third Meeting of the British Association for the Advancement of Science, Held at Nottingham in September 1893* (London: John Murray, 1894), pp. 6–7.

been turned over to the special disciplines for solution. On the other hand, the ecologist is continually drawing on the methods and resources of physiology, morphology, taxonomy, distribution, etc., in solving his own particular problems of adaptation and behavior. The most interesting and important of them relate, not to the reactions of organisms to their inorganic environment, but to their reactions to one another. As this matter, though very simple, is often misunderstood, you will pardon me for dwelling on it for a few moments.

Since all organisms, either of the same or of different species, invariably live in relationships of dependence on or of coöperation with others, the ecologist is justified in regarding the whole living world as an intricate congeries of biocoenoses, or consociations, ranging in complexity from at least two to a great many organisms. Even genetics may be regarded as a department of ecology, which is striving to formulate the precise symbiotic relationships of the gametes to each other in the constitution of the zygote, and their reactions with the environment. Hence the problem of adaptation is not foreign to this discipline though it is at present either ignored, as Bateson implies, or expressed in terms that are unfamiliar to the ecologist and physiologist. Moreover, since human societies are very intimate and elaborate biocoenoses of individuals of the same species, psychology, sociology, economics, anthropology, ethnology, history, ethics, jurisprudence, government, hygiene, medicine, etc., are essentially ecological, for their central problems are behavioristic.

It follows from these considerations also that applied or economic biology is merely applied ecology, as Forbes, Need-

ham, and others have repeatedly stated.[9] Whenever and wherever one of the organisms of a biocoenose happens to be man, we have an economic situation, and it is in the precise determination of the relationships thus developed that ecology celebrates many of its greatest triumphs. I need only refer to the great field of parasitology — the work on cestodes, trematodes, trichinæ, hookworm, malaria, yellow fever, and all the other insect-borne pathogenic organisms, in bacteriology, phytopathology, economic entomology, etc., all work which does not transcend the concrete natural history or ecological level. And everything indicates that we are only at the beginning of the revelations and benefits which similar studies have in store for us. Surely the ecologist need not veil his face in modesty even in the presence of a Mendelian formula or a new Drosophila mutation.

Although I have left our lusty young freshmen out in the cold during this long harangue, I have not forgotten them.

[9] Cf. the following passage by Professor J. G. Needham, *Science*, N.S., XLIX, 457 (1919): "Dr. Howard suggests that we give more time to taxonomy and ecology and less to physiology and genetics. This is a good suggestion. We are all out of balance. Some of our laboratories resemble up-to-date shops for quantity production of fabricated genetic hypotheses. Some of our publications make a prodigious effort to translate everything biological into terms of physiology and mechanism — an effort as labored as it is unnecessary and unprofitable. Why not let the facts speak for themselves? Our laboratories are full of fashions. They go from one extreme to another. In my high school days we learned systems of classification; in my college days we did nothing but dissecting; later came morphology and embryology, then experimental zoology, then genetics, and the devotees of each new subject have looked back upon the old with something like that disdain with which a debutante regards a last year's gown. Natural history and classification are perhaps long enough out of date, so that interest in them may again be revived. I hope so; for these are the phases of biology by means of which a youth is best oriented for more special work. Then, too, they are immensely practical. One has to deal with species, and must be able to recognize them; and all economic procedure is applied ecology."

I repeat: what ought we to give them? I do not believe that we should inform them with the first crack out of the box that they are animals and descended from ape-like ancestors. This must come as a severe shock to any young *Boobus americanus* who has never had an opportunity to make the acquaintance of really high-class apes, like the chimpanzees recently studied by Wolfgang Koehler at the German Anthropoid Station on the Island of Teneriffe. The freshman should be gradually led through a sympathetic study of the lower organisms as marvelous centers of beautiful and dignified processes to a knowledge of his own animal respectability, descent, and responsibilities. This, I am convinced, is not to be achieved by taking dead and more or less smelly crayfish, earthworms, starfish, and cockroaches to pieces, because Huxley in 1879 intimated that it might be a meritorious occupation for the young, nor by a too immediate study of living forms so remote in the scale of being as the Protozoa, Coelenterates, and plants. It would seem to be preferable to start with living animals somewhere in the middle or higher reaches of organic development — small vertebrates, mollusks, insects, arachnids — and to make them the objects of direct, simple, comprehensive observation and experiment, severely suppressing or subordinating all morphological details which have no immediate bearing on the study of their activities. Necropsies, autopsies, and postmortems might be introduced with discretion, but only after the student has acquired an acquaintance with the life-histories and more obvious methods of growth of his organisms — with the aid of moving pictures, whenever necessary — their methods of locomotion, feeding, respiration, excretion, defense and concealment, their reactions to light, tem-

perature, humidity, etc., and especially to one another, that is to say, their mating, oviposition, parturition, nidification, parental care, predatory, parasitic, symbiotic, gregarious and social behavior, etc. Simple experiments in genetics, regeneration of lost parts, etc., could be introduced, but without cytological lace and ruffles. The successful teacher of elementary mathematics does not overwhelm and confuse the student with all the known recondite properties of the triangle and circle. The freshman laboratory should be neither an animal morgue nor a herbarium, but a vivarium. Its teaching staff should be numerous, competent, enthusiastic, and young and, in order that Merulius infection may be avoided, no old professor or weary research student should be permitted to enter it without a complete change of mental underwear and, I might add, without a few moments of silent prayer or meditation at the door. To the present depauperate glacial fauna of the laboratory, the perpetual rat-guinea-pig-frog-Drosophila repertoire, we should add many of the thousands of even more interesting organisms that will live and multiply in confinement, and — although I realize the great difficulties involved — some means must be devised for taking the students into the field more frequently, since it is impossible to reproduce and study the more complex biocoenoses under artificial conditions.

You will probably agree that such a program of freshman work as I have very hastily sketched could in adroit hands yield at least a vital part of the needed preparation, first, for men who will devote the remainder of their collegiate and postcollegiate lives to occupations foreign to biology, and such men, of course, constitute the majority of any freshman class; second, for men who are primarily interested in the

"Geisteswissenschaften" — psychology, philosophy, history, economics, law, etc.; third, for men who will enter medicine and may therefore be expected to specialize mainly in morphology and physiology during the remainder of their college course; fourth, for men who may wish to specialize in other departments of applied biology, such as agriculture, forestry, economic zoology and botany, fish and game conservation, etc., subjects to which our present freshman biology is a hopelessly inadequate introduction; fifth, for the biological investigator and teacher, who cannot be too quickly persuaded to assume the modern dynamic and experimental attitude toward his science. It is, of course, this new attitude that many of us older men, trained during the late Victorian morphological boom, have difficulty in assuming, and that makes us so conscious of our inability to participate very effectively in the biological education of the present generation.

There is another suggestion I should like to make, in order that the freshman course may be preserved from the dry-rot which may invade even the most dynamic type of instruction, and that is the utilization by the instructor of competent amateur naturalists as occasional assistants. This seems never to have been tried, except in some of our summer camps and marine laboratories, and the reason is obvious. The typical professor has about the same liking for the amateur that the devil has for holy water, and the amateur habitually thinks of the professor in terms which I should not care to repeat. You will find a choice collection of them in Mencken's writings. The truth is that the amateur naturalist radiates interest and enthusiasm as easily and copiously as the professor radiates dry-rot. For years I have taken a malicious delight in introducing amateurs to professors, be-

cause the behavior of the latter on such occasions yields a precise quantitative test of the amount of Merulius in their timber. Dear old, mellow, disinfected professors of the type of Louis Agassiz, Asa Gray, Shaler, Hyatt, and Ryder enter at once into sympathetic rapport with the humblest amateur, but the young or those of middle age are almost invariably more or less priggish, condescending, or worse. Now there is an opportunity to develop a mutual understanding and respect in both of these parties, so essential to the development of biological science, if the young instructors will only welcome and encourage the coöperation of the amateur in interesting his freshmen. We have all known amateurs who could make an enthusiastic naturalist out of an indifferent lad in the course of an afternoon's ramble and, alas, professors who could destroy a dozen budding naturalists in the course of an hour's lecture. The instructor who would from time to time call in some of our talented ornithologists, herpetologists, entomologists, arachnologists, and malacologists to assist him, both in the laboratory and the field, would himself profit greatly; the significant human contacts of the students would be multiplied and the amateur be given just the right environment in which to spread the divine fire of his enthusiasm.

And this brings me in conclusion to what is perhaps the main source of our failure in incubating naturalists, and that is our too highly specialized or esoteric attitude toward organic nature. Whether we contemplate the whole or only some particular portion of the realm of living things, it eventually tends to become for us merely so much material to be used in the solution of the many tantalizing problems which it suggests. We are, indeed, obsessed by problems. No doubt this is the correct attitude for the seasoned investigator,

and no doubt a certain spirit of skeptical inquiry should be cultivated even in freshmen, but surely we should realize, like the amateur, that the organic world is also an inexhaustible source of spiritual and esthetic delight. And especially in the college we are unfaithful to our trust if we allow biology to become a colorless, aridly scientific discipline, devoid of living contact with the humanities. Our intellects will never be equal to exhausting biological reality. Why animals and plants are as they are we shall never know, of how they have come to be what they are our knowledge will always be extremely fragmentary, because we are dealing only with the recent phases of an immense and complicated history, most of the records of which are lost beyond all chance of recovery; but that organisms are as they are, that apart from the members of our own species they are our only companions in an infinite and unsympathetic waste of electrons, planets, nebulæ, and suns, is a perennial joy and consolation. We should all be happier if we were less completely obsessed by problems and somewhat more accessible to the esthetic and emotional appeal of our materials, and it is doubtful whether, in the end, the growth of biological science would be appreciably retarded. It quite saddens me to think that when I cross the Styx I may find myself among so many professional biologists, condemned to keep on trying to solve problems, and that Pluto, or whoever is in charge down there now, may condemn me to sit forever trying to identify specimens from my own specific and generic diagnoses, while the amateur entomologists, who have not been damned professors, are permitted to roam at will among the fragrant asphodels of the Elysian meadows, netting gorgeous, ghostly butterflies until the end of time.

VII

EMERGENT EVOLUTION AND THE DEVELOPMENT OF SOCIETIES[1]

We have hitherto failed in our comprehension of life mainly because we have been involved in the absolute method of dealing with things — have been more intent on discovering what units are for themselves than on finding out how they are related to and influenced by the systems to which they belong. — NOBLE, *Purposive Evolution*

Toutes les fois que des éléments quelquonques en se combinant, dégagent par le fait de leur combinaison des phénomènes nouveaux, il faut bien concevoir que ces phénomènes sont situés non dans les éléments, mais dans le tout formé par leur union. Appliquons ce principe à la sociologie. Si, comme on nous l'accorde, cette synthèse *sui generis* qui constitue toute société des phénomènes nouveaux, différents de ceux qui se passent dans les consciences solitaires, il faut bien admettre que ces faits spécifiques resident dans la société même qui les produit, et non dans ses parties, c'est-à-dire dans ses membres. — DURKHEIM, *Les Règles de la Méthode Sociologique*

Notez qu'étudier les individus ne veut pas dire que l'on doit considérer plusieurs de ceux-ci mis ensemble comme une simple somme; ils forment un composé, lequel, à l'égal des composés chimiques, peut avoir des propriétés des composants. — PARETO, *Traité de Sociologie Générale*

THE WORDS "emerge," "emergent," and "emergence" are now employed so frequently and have sometimes been employed so loosely that it behooves an author to define the precise meaning he wishes them to convey. "To emerge" may, of course, signify that something comes up

[1] This paper in its original form, with the title "Emergent Evolution of the Social," was one of four addresses in a symposium on Emergence held at the Sixth International Congress of Philosophy, Cambridge, Mass., September 14, 1926, and was published in the *Proceedings* of the Congress during 1927. It had previously appeared in *Science*, November 1926, and in *Psyche*, January 1927. Subsequently the introduction was rewritten,

out of a liquid after being immersed, like a grebe or a loon out of a lake, or that something comes into view after concealment, like the sun when the clouds disperse. The verb is also occasionally used of facts that are revealed as the result of an inquiry or of questions or difficulties that suddenly crop up. In a more technical sense it is employed by entomologists when they say that a butterfly "emerges" from its chrysalis (in the sense of the German *entpuppen*), in order to avoid the word "hatch," which they restrict to the issuing of the caterpillar from the egg. In all these cases, except that of a difficulty, the something emerging is supposed to preëxist and merely to become visible or manifest. The difficulty said to "emerge" or "arise" leads naturally to a consideration of "emergency," which does not connote preexistence but the sudden and unexpected outcome of a critical, or contingent constellation of events, or happenings calling for immediate intervention. The two meanings now conveyed by the words "emergence" and "emergency" might therefore be called the preformational and the epigenetic respectively. They were not clearly distinguished by some of the writers of the past century, or at any rate the words were sometimes used interchangeably, and this is true also of the adjective "emergent" and its noun derivative. There is a third meaning of "emergence" or "emergency" which is

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additions were made to certain other paragraphs in an endeavor to clarify or illustrate the author's meaning, the bibliography was somewhat expanded, and the paper was published in a small volume, *Emergent Evolution and the Development of Societies* (New York: W. W. Norton & Co., Inc., 1928), together with a second part calling attention to certain historical statements on emergence and to the works of a number of contemporary authors holding similar views. The second part is here omitted. Permission to reprint has been granted by The Science Press and W. W. Norton & Co., Inc.

so epigenetic as to deserve the epithet miraculous. Kallen, in his admirable book, *Why Religion*, states that "Mary Morse Baker Glover Patterson Eddy, the founder of the (Christian Science) sect, records that she had been healed of an incurable disease by 'emergency into the light.'" Mrs. Eddy may have meant that, after preëxisting as an incurably diseased metaphysical chrysalis, she suddenly burst into the light as a healthy metaphysical butterfly, though it seems more probable that she uses "emergency" in the sense of a miraculous change of belief. But all exegesis in such cases is apt to take too much for granted. In religious writings words should always be used in such a way as to connote the greatest possible number of meanings, so that the theologians may not lack for employment.[2]

Now "emergence," in the following pages, signifies neither the manifestation or unveiling of something hidden and already existing, as in the common and the entomological denotations of the word, nor some miraculous change, as in Mrs. Eddy's conversion, but a *novelty of behavior* arising from the specific interaction or organization of a number

[2] Indeed, the writers of the most successful sacred texts, such as the Bible, the Koran, the Book of Mormon, and Science and Health, have subconsciously conformed to this practice, which was long ago psychoanalyzed by Lucian in *The Dream, or the Cock*. The cobbler Mycillus is astonished to hear his cock suddenly remark in excellent Greek that he is a reincarnation of Pythagoras. Of course the cobbler cannot help asking him why, if he is Pythagoras, he has been eating the beans that were cast before him, since that worthy had made the eating of beans *taboo*. The cock is ashamed and embarrassed, but finally confesses: "There was no sound or good reason, but I saw that if I should believe what was customary and the same as the masses, I would, in a very small measure, draw men to the wonder. The more odd I might be, the more reverence I thought I would receive from them. On this account I decided to introduce something new, keeping the cause of it a secret, in order that one conjecturing in one way and another in another, they might all be amazed just as they are in the mysteries of the oracles." (D. C. Brown's translation.)

of elements, whether inorganic, organic, or mental, which thereby constitute a whole, as distinguished from their mere sum, or "resultant." The classical example is, of course, such a chemical compound as H_2O, in which hydrogen and oxygen combine under certain conditions and in certain proportions to form a liquid emergent, water, exhibiting a very different behavior (properties) from that of either of its gaseous components. In this sense, "emergence" acquires the epigenetic meaning of "emergency," and G. H. Lewes (1875), who was the first to use the word to designate such chemical behavior, probably had this meaning in mind.[3] The distinction between sum and emergent had been previously recognized by J. S. Mill (1843), who derived the latter from "heteropathic causation." L. F. Ward and Spaulding prefer "creative synthesis." Wundt uses the term "creative resultants," Sellars "evolutionary naturalism," C. L. Morgan "emergent evolution," Broad "emergent vitalism," Smuts "holism," L. J. Henderson and others "organicism." I have adopted C. L. Morgan's term because it seems most applicable to the matters which I wish to discuss.

Emergence, as above defined, is a favorite concept, especially with a number of contemporary American and British realistic philosophers and biologists, including Holt, Spaulding, Sellars, Alexander, C. L. Morgan, Gordon, C. K. Ogden, G. H. Parker, and Jennings, all of whom have stressed the unique qualitative (that is to say, behavioristic) character of organic and inorganic wholes as due to the peculiar non-additive relations or interactions among their parts.

[3] "There are many ways in which the properties of a mass differ from those of its molecules; the chief of them is, that some properties are *emergents*, not resultants."

Since the various sciences are concerned with the investigation of wholes of different degrees of complexity, this conception is, perhaps, implicit also in Comte's hierarchy of the sciences, to which we still adhere, and in our various chemical and biological classifications. According to Spaulding,

certain specific relations, recognized, named, and *technically formulated in special sciences, organize* parts into wholes, and there are states of affairs resulting [we should now say "emerging"] that are identical with new properties, and that are different and distinct from the individual parts and their properties. Therefore the *reduction* of these new properties to those of the parts *in the sense of identification*, and the finding of a *causal determination* also in this same sense is *impossible*. The properties of the whole are, at least some of them, new, and in just this respect *are a "law unto themselves"* and *in this sense free*. This does not mean that they are lawless, but only that their specific principles of "behavior" are not identical with those of the parts.[4]

[4] This matter has been more fully considered by Broad (*The Mind and Its Place in Nature*, New York: Harcourt, Brace & Co., 1925, p. 77): "On the emergent theory we have to reconcile ourselves to much less unity in the external world and a much less intimate connection between the various sciences. At best the external world and the various sciences that deal with it will form a kind of hierarchy. We might, if we liked, keep the view that there is only one fundamental kind of stuff. But we should have to recognize aggregates of various orders. And there would be two fundamentally different types of law, which might be called 'intra-ordinal' and 'trans-ordinal' respectively. A trans-ordinal law would be one which connects the properties of aggregates of adjacent orders. A and B would be adjacent, and in ascending order, if every aggregate of order B is composed of aggregates of order A, and if it has certain properties which no aggregate of order A possesses and which cannot be deduced from the A properties and the structure of the B-complex by any law of composition which has manifested itself at lower levels. An intra-ordinal law would be one which connects the properties of aggregates of the same order. A trans-ordinal law would be a statement of the irreducible fact that an aggregate composed of aggregates of the next lower order in such and such proportions and arrangements has such and such characteristic and non-deducible properties. If we consider the properties of a given aggregate of high order we could then divide them into three classes. (i) Those which

And, paraphrasing the dictum that to be determined by one's own nature is to be free, he adds,

> *Freedom consists, therefore, of action in accordance with those characteristics which subsist at a certain level of organization* but do not exist at other (lower) levels, yet is quite compatible with law and determination both at this higher level and at lower levels. Freedom of this kind subsists *at each level* of reality in the universe, not only in the mental but also through the physical and the merely subsistent realms.

It is perhaps unnecessary to point to the essential similarity between emergence as thus understood and the *Gestalt* of the configurationists, Wertheimer, Koehler, Koffka, Drexler, and others.

There is evidently danger of conceiving the emergents in a fashion too schematic, too rigid, and too static. The whole constituted by the organized and integrated parts need not be regarded as novel in its entirety. The novelty is variable and may appear only in certain functional aspects of the whole. Since wholes have a manifest cumulative tendency to combine and recombine to form ever more complicated wholes, the ascending hierarchy of emergents has been much stressed. It is far from being universal, however. By loss

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are characteristic of this order, in the sense that all aggregates of the order possess them, that no aggregate of lower order does so, and that they cannot be deduced from the structure of the aggregate and the properties of its constituents by any law of composition which has manifested itself in lower orders. These might be called the 'ultimate characteristics' of the order. (ii) Those which are characteristic of this order; but which could in theory be deduced from the structure of the aggregate, the properties of its constituents, and certain laws of composition which have manifested themselves in lower orders. These might be called 'reducible characteristics' of the order. (iii) Properties which aggregates of this order share with those of lower orders. These might be called 'ordinarily neutral properties.' "

or simplification of parts or suspension of some of their interactions, there is also an *Abbau*, or unbuilding, productive of simpler emergents. This is clearly seen in the many recessive mutations of plants and animals and in the numerous secondary simplifications of such forms as parasites and other highly specialized organisms which are demonstrably descended from more complicated and nevertheless more primitive and generalized ancestors. There is, therefore, an evolution by atrophy as well as an evolution by increasing complication, and both processes may be going on simultaneously and at varying rates in the same organism. We must also remember that most authors artificially isolate the emergent whole and fail to emphasize the fact that its parts have important relations not only with one another but also with the environment and that these external relations may contribute effectively towards producing both the whole and its novelty. This tendency to abstraction has led Professor H. C. Brown to remark that

> the whole is then physically more than the sum of the parts *we have taken account of*, although these may be legitimately abstracted as sufficient for correlation with the consequences of the integration. The extraordinary sense of mystery some seem to feel about the process seems to me to arise from forgetting this interstitial filling that is as real as the selected elements and renders technically false the literal interpretation of the appealing paradox of the whole that is something more than the sum of its parts.

If all wholes of which the parts are organized, or exhibit those relations which we call integration, differentiation ("division of labor"), interactive accumulation, etc., among themselves and with the environment, are emergents,

we must agree with Morgan that "it is beyond the wit of man to number the instances of emergence." Since no two events are identical, every atom, molecule, organism, personality, and society is an emergent and, at least to some extent, a novelty. And these emergents are concatenated in such a way as to form vast ramifying or sympodial systems, only certain ideal sections of which seem to have elicited the attention of philosophers, owing to their avowedly anthropocentric and anthropodoxic interests. These sections have been called levels. The word is not very apt since it conveys a spatial and static metaphor, whereas emergents must be regarded as intensively manifold spatiotemporal events. Naturally no two authors agree in their lists of levels. Metaphysicians and epistemologists like Alexander and Morgan are mainly interested in space-time, matter, life, mind, and deity as successive emergent levels. To the biochemist, biophysicist, biologist, and physiological psychologist, however, life and mind are so amazingly complex and comprise so many heterogeneous processes that their blanket designation as two emergent levels cannot seem very illuminating, and to the observer who contemplates the profuse and unabated emergence of idiots, morons, lunatics, criminals, and parasites in our midst, Alexander's prospect of the emergence of deity is about as imminent as the Greek kalends.

Our knowledge of organisms and their development will hardly permit us to accept such levels as life and mind as having been established *uno ictu*. Emergence must be more ambulatory, or at any rate less saltatory. If the naturalist is to accept both genetic continuity and novelty in evolution, the viable novelty at each emergence must be very

small indeed.[5] This is attested both by the extraordinary slowness of phylogeny and the very subtle transitions in even the most rapid ontogenies. Even metamorphosis in organisms is only superficially saltatory. Novelties such as life and mind, conceived in a wholesale fashion, are of such magnitude that we can regard them only as representing the final accumulative stages of a very long series of minimal emergences. The insistence on levels becomes, therefore, largely a matter of descriptive emphasis and should not conceal the necessity for detailed scientific knowledge of every emergence and the peculiar constellations and interactions of the parts which immediately determine it.

One of the levels in which the situation, as it appears to me, is most open to investigation, is the social. Unfortunately the subject has been passed over by writers on levels with only a few vague remarks. Unfortunately, also, the science of comparative sociology has remained undeveloped. It has, in fact, fallen between two stools, because the sociologists have left the study of animal and plant societies to the biologists and the latter have been much less interested in these societies as such than in the structure and individual activities of their members. Apart from Forel and myself only a few investigators, like Espinas, Waxweiler, Petrucci, Deegener, and Alverdes, have evinced a keen interest in non-human societies. Yet these, no less than human society, are as superorganisms obviously true emergents, in which whole organisms, that is, multicellular organisms, function as the interacting determining parts. Owing, moreover, to the loose and primitive character of the integration and the

[5] I have used the word "viable" intentionally because monsters and other extreme unadapted mutations are also emergents.

size of the components even in the densest societies, it is possible to ascertain the behavior of the parts and to experiment with them more extensively than with chemical and organismal wholes, since the parts of the latter are either microscopic or ultramicroscopic and are always so compactly integrated that analysis becomes very difficult and involves a considerable amount of statistical inference. Experiments in subdividing, compounding, castrating, and grafting, and in introducing foreign elements with a view to observing their effects on animal and plant societies as emergent wholes, can be carried far beyond the limits of such experiments on the single living organism. For this reason, for the reason that there is a much greater wealth of emergents at the animal social level than is commonly supposed, and because the peculiarities of social emergence bear an interesting analogy to those of mind, the reader will pardon me if I descend to a rapid review of a number of biosociological details.

Social aggregates — if we employ the term "social" in its broadest sense — may be divided into two great groups, the heterogeneous and the homogeneous, the former comprising the associations of organisms belonging to different species, the latter of individuals of the same species and therefore of common genetic origin. In either group the simplest association obviously obtains between two interacting individuals, the combined behavior of which may be said to form an emergent pattern different from, though depending on, the functional peculiarities of the two component organisms. Among the heterogeneous associations we can distinguish the innumerable cases of predatism, parasitism, symbiosis, and the biocoenoses, or animal and

plant communities, which constitute a vast series of emergents varying from those of very low to those of very high integration. In predatism the predator becomes structurally and behavioristically adapted to the prey and the latter to the predator, at least to the extent of modifying its habits of flight, concealment, defense, or fecundity. In these cases we can hardly speak of association in the social sense, but it may be noted that if the predatory species indulges in too great an extermination of the prey, it must either adapt itself to some other form of prey or automatically cease to exist. In parasitism this danger is the greater because the association of host and parasite is so close as to be usually one of actual bodily contact. Moreover, the parasitic association which is exhibited, either temporarily or permanently, by many thousands of animal and plant species, tends to ever greater definiteness through the selection of specific hosts by the parasites. This type of association is unilaterally aggressive like predatism, but tends in turn to lapse into a relation of mutualism, or symbiosis between the interacting individuals, again giving rise to innumerable emergents exhibiting such diverse behavioristic wholes as the symbiosis (helotism) of alga and fungus in the lichens, the association of bacteria or Mycorrhiza with the roots of higher plants, the singular associations of pollinating insects with flowers, between the yeasts or bacteria and the tissues of plant lice and other insects, between ants and certain tropical trees and shrubs, the cultivation of fungi by beetles, ants, and termites, etc. Recent studies like those of Raines (1922) on the rusts and their host-plants and of Melin (1925) on Mycorrhiza and the roots of forest trees, show the various predatory, parasitic, and symbiotic associations as emergents

in statu nascendi. Finally there are the biocoenoses, or associations of plants and animals that live in particular environments, such as swamps, deserts, rain-forests, et cetera — veritable welters of organisms of many species, all interacting with one another in complex predatory, parasitic, and symbiotic relationships, but forming wholes in which the experienced field-naturalist can readily distinguish general adaptive patterns, though their adequate description may be impossible. In the tropics a single species of tree may harbor and nourish more than a hundred species of insects peculiar to itself, and these may, in turn, be the prey of many predatory insects, reptiles, birds, and mammals and the hosts of innumerable fungus, protozoan, vermian, and insect parasites. We may truthfully say that there is not on the planet a single animal or plant that does not live as a member of some biocoenose.[6]

True societies are possible only when the components belong to the same species, but the motives of their association may be very diverse. They may be said to belong to three main types according as nutritional, reproductive, or defensive functions predominate in the emergent social behavior. Examples of the nutritional type are certain Coelenterates, like the Siphonophores, corals, etc., the tunicates, tapeworms, and the higher vascular plants. In all these cases the society, or colony, is formed asexually by repeated budding from a single individual, and sexual reproduction is restricted to the

[6] Classical examples of biocoenoses are the clover–bumblebee–fieldmouse–cat–spinster community of Darwinian literature and the maize-plant–corn-root-aphis–ant–horse–man community of our Midwestern corn belt. For more intricate examples and an interesting discussion of the biocoenoses from the ecological and sociological points of view, see Elton, *Animal Ecology* (New York: Macmillan, 1927).

dissemination of the species and the formation of the initial individual of the colony. Certain members of the colony may be specialized for the purpose of securing food, but this is shared by all the vitally interconnected individuals.

The sexual, or reproductive, type of society is more interesting. It starts with a peculiar temporary coöperation, or mating of only two individuals, the male and the female, and emerges with the growing up of the offspring in cooperative affiliation with the mother or with both parents. A more or less permanent family is thus formed, which may become very numerous either through the production of successive generations of offspring by the same mother or through the consociation of a number of genetically related mothers and their offspring. This is the type of society which we find among the insects, and I have been able to show that it has emerged at thirty independent points at least during the phylogeny of the class. Some of these colonies are very small and evanescent or feebly integrated but others are very stable, comprise many thousands of individuals (ants, honeybees, social wasps, and termites) and are very highly integrated, with so pronounced a social division of labor among the individuals that definite castes are produced (workers, soldiers, etc.) which are not only functionally, but may even be morphologically differentiated at an early stage of their post-embryonic development. Although the formation of the various castes is primarily connected with the functions of nutrition and defense, the main activities of the colony center in reproduction, that is to say, in producing and rearing as many young as possible. That the social activities may present a very definite emergent pattern is most clearly seen in the nests of bees, wasps, ants,

and termites. These structures, though the result of the co-operative labor of most of the personnel of the colony, are nevertheless true *Gestalten*, being no more mere sums of the individual activities than is the diverse architecture of cities built by human hands. Not only does each species have its peculiar type of nest, but the nest of every colony of a species exhibits its own emergent idiosyncrasies.

The situation among the social insects may be complicated in a very interesting manner by the tendency of their colonies to adopt alien insects as guests, or nestmates. This is especially true of the ants and termites. These guests are really social parasites and are to be regarded as component members of the colonies in the same sense as dogs have for ages been effective members of human societies. But in the case of the social insects the behavior of the guests may produce veritable social diseases in the colonies that harbor them. As a result of their adoption even the structure and numerical proportions of the castes may be modified, although there is a demonstrable effort at social regulation on the part of the host, just as there is in the single organism whose tissues have been invaded by bacteria or other parasites. This tendency to consociation with strange organisms is carried even further in the union of whole colonies of bees, wasps, and ants with colonies of alien species, and in these so-called "mixed colonies" one of the social components assumes a predatory or parasitic role, suppressing the fertile queen, or reproductive organ of the host colony, that is to say, indulging in what is known as "parasitic castration" among single organisms, and controlling the activities of the whole, so that a new emergent arises — a super-superorganism, or superorganism of the second degree.

The defensive colonies are represented by the schools, flocks, herds, and bands of fishes, birds, and mammals and consist of individuals, sometimes of only one sex or of the young, belonging to different families. While these congregations are usually based on sexual reproduction, their primary social function is nevertheless the protection of the individuals. There is often a vague differentiation of function as in the stationing of sentinels or of more vigorous or more formidable individuals in strategic positions when the herd is feeding or is otherwise exposed to danger. The bands of monkeys, anthropoids, and primitive men constitute loose social aggregates of this pattern. We must, I believe, regard human societies above the level of the primitive savage horde as still higher emergents, that is, as super-superorganisms which not only have their reproductive bases in the consociation of numerous families, but have developed innumerable groups, or associations, all so inextricably interrelated that a single individual not only has multitudinous relations with the members of his own and other families, but may belong simultaneously to a number of different associations. The total emergent functional pattern is here so amazingly complicated that it altogether defies observation as a whole. Only in certain cases, such as ceremonies and rituals, is it possible to observe emergent social patterns as wholes, or *Gestalten*. A fine example of such a pattern, covering a wide area and many activities and carried out by many individuals, though unperceived by the latter, is the *kula* among the natives in the archipelagoes of Melanesian New Guinea as described by Malinowski in his *Argonauts of the Western Pacific*.

Whereas nearly all insect societies possess an ontogeny, since

they have their inception in a single fertilized mother queen and exhibit a gradual growth, integration, and differentiation as new individuals are successively added from the eggs of the queen till the colony attains a definite adult stature in a manner analogous to that of the ontogeny of the single organism by division and differentiation of its component cells, human society no longer possesses an ontogenetic stage but grows indefinitely by a kind of interstitial swarming, which resembles that of the honeybee only when contingents of individuals are sent out as colonies, as occurred in ancient times among the Greeks and Romans and has been the practice of other European nations during more recent centuries.

Now the various emergents which I have hastily discussed indicate that there is something fundamentally social in living things, and closer scrutiny shows that this must be a characteristic of all life, since every organism is, at least temporarily, associated with other organisms, even if only with members of the opposite sex and with its parents, and every organism is at least implicated in some biocœnose. This statement holds good even of such supposedly unsocial creatures as lions, eagles, sharks, tiger-beetles, and spiders. There are, in fact, no truly solitary organisms. We may say, therefore, that the social is a correlate as well as an emergent of all life in the sense in which Morgan speaks of the mind as being both a correlate and an emergent of life. And like the more complicated mental emergents, such as the instincts and conscious activities, striking social emergents make their appearance sporadically and often in unrelated groups of species, as I have shown among the insects.[7] In-

[7] This matter is fully discussed in my *Social Life Among the Insects* (New York: Harcourt, Brace & Co., 1923) and *Insect Societies* (International Library of Psychology, etc., 1928).

deed, the correlations of the social — using the term in its most general sense — even extend down through the non-living to the very atom with its organization of component electrons. And since reality is given as classes of elements, each represented by innumerable similar, active entities, endowed with an irresistible tendency to cohere and organize themselves into more and more complex emergent wholes, association may be regarded as the fundamental condition of emergence. We are, I believe, bound to assume that the organization *is entirely the work of the components themselves* and that it is not initiated and directed by extraspatial and extratemporal "entelechies" (Driesch), "organizatory factors" (Eldridge), "deity" (Alexander), or "*élan vital*" (Bergson). Such agencies are conceived as possessing remarkable foresight, although the whole multimillennial course of evolution with its innumerable *impasses* and *culs-de-sac*, its abject and tragic failures, would seem rather to be one vast monument to their colossal and hesitating inadequacy, blindness, and stupidity. The resort to such metaphysical agencies has been shown to be worse than useless in our dealings with the inorganic world and it is difficult to see how they can be of any greater service in understanding the organic. The tender-minded may still delight in assuming their intervention in the development and maintenance of unicellular and multicellular organisms, whose integration is so exceedingly complicated and opaque that we are probably still centuries removed from any adequate understanding of their functional composition, but on the next level, that of the very loosely organized social, or superorganisms, in which the actual play of the components is open to inspection, it is not so easy to tolerate these ghostly presences.

I fail to understand why Alexander and Morgan select deity as the supervenient level next to mind, since their general scheme of emergent evolution most naturally demands the social as the next level in ascending order. As I. A. Richards says in *Science and Poetry*:

> Various emergent deities have been suggested — by Mr. Wells, by Professors Alexander and Lloyd Morgan — but, alas! the reasons for suggesting them have become too clear and conscious. They are there to meet a demand, not to make one; they do not do the work for which they were invented. The revolution brought about by science is, in short, too drastic to be met by any such half-measures. It touches the central principle by which the Mind has been deliberately organized in the past, and no alteration in beliefs, however great, will restore equilibrium while that principle is retained.[8]

Were prophecy in order we might ask what level may be expected to emerge beyond the social. Perhaps this may be the end of the series, with supervenient extinction, also to be accepted by the race with good cosmic manners if not with Morgan's "natural piety." It would seem, however, that the present very imperfect state of our society may allow for not a few successive emergents in the form of greater solidarity and higher ethics. But here we touch on a consideration which even Herbert Spencer felt to be ominous. Will this prospective, more intensive socialization be analogous to that of the highest social insects, a condition in which specialization and constraint of the single organism are so extreme that its independent viability is sacrificed to a system of communal bonds, just as happens with the individual cell in the whole organism?

[8] *Science and Poetry* (New York: W. W. Norton & Co., Inc., 1926), p. 60.

Within the groups of social insects, as we pass from the socially primitive to the more specialized, or in the ontogeny of the single colony as we pass from its earlier to its later stages, we actually witness a notable and increasing degeneration of the individual. Holmgren has shown that the supraœsophageal ganglion, or brain, in mature kings and queens of termites shrinks to one-third, while the sympathetic ganglia increase to three times their original size. And von Rosen finds that the eyes and optic ganglia of aged royalty among termites also undergo marked degeneration. Furthermore, the eyes, brain, thoracic structure, and often also the pigmentation of the workers in the most highly socialized ants are less developed than they are in the workers of small primitive societies, which are more like the original solitary Vespoid ancestors. There may even be a complete suppression of the worker caste and a return, or dedifferentiation, to what is to all intents and purposes a nonsocial life in certain ants, bees, and wasps (Anergates, Psithyrus, *Vespa austriaca*, etc.) which behave as parasites in the colonies of other ants, bees, or wasps. We also notice a concomitant degeneration in pigmentation and other structures as we pass from primitive forms like Calotermes, Archotermopsis, and Hodotermes, with their small colonies, to Termes with its huge colonies of highly specialized individuals. Many more examples might be cited, but these will suffice to show that evolution by atrophy certainly accompanies an advance in social integration in the insects.

Turning to man we notice a similar regressive development of the individual as civilization proceeds. There is a decline in the sense-organs (witness the number of people with congenital or acquired defects of vision, hearing, and

smell), anomalies in the epidermal structures (teeth, hair, and pigmentation), the absence of any demonstrable improvement in the brain cortex and intelligence during historic time (possibly even some deterioration!), the greater activity of the visceral nervous system and endocrine glands as shown by the higher emotivity, increasing insanity, criminality, and mob psychology in our larger cities, et cetera. Add to all this the atrophy or subatrophy of our organs and tissues brought about by the ever-increasing specialization in our activities, and we can hardly fail to suspect that the eventual state of human society may be somewhat like that of the social insects — a society of very low intelligence of the individuals combined with an intense and pugnacious solidarity of the whole. Even the intensification of nationality witnessed in existing human society has its counterpart in the hostility of every colony of social insects towards every other colony, even of the same species. A society of the type towards which we may be drifting might be quite as viable and quite as stable through long periods of time as the societies of ants and termites, provided it maintained a sufficient control of the food supply. Intersocietal hostility undoubtedly has its origin in the mutual hostility of the individuals, both of the cells or tissues of single organisms and of the individual organisms composing superorganisms, and this "hostile symbiosis" is the foundation on which Morley Roberts has recently erected a very interesting theory of evolution. The primitive predatism and parasitism from which symbiosis has emerged is not lost by the individuals composing organisms or societies but merely abides in latency, as the most casual observation of our species demonstrates, ready to flare up under certain conditions with the most dis-

astrous results, such as the death of the individual (cancer and other malignant growths and variations), or the dissolution of society (revolution). It follows from such considerations that the optimistic conception of progress as an unceasing process in the human race may be illusory. Roberts suggests that our enlarged fore-brain, the "specific organ of civilization" (C. J. Herrick), with its ninety-two hundred million neurons, of which we are so proud, may really have originated as a malignant overgrowth (tumor), and he remarks that

> in discussing the factors of evolution objections to our regarding the encroachments of the fore-brain upon the animal function of the human body as perpetual approximations to and recessions from a state of morbid overgrowth, on the ground that to this we owe human progress, are wholly irrelevant. Progress, whatever it may be, is obviously relative, and a healthy Neanderthal or Cromagnon man, who might as easily dispose of a modern athlete as any gorilla, could be held excused if he thought his bald and almost jawless successor to be in the highest degree degenerate. There can be no doubt that what we, perhaps in our blindness, call the upward progression of the human race, has always been accompanied, especially when advance seemed most rapid, by an increase in disease, and it would in no way be surprising if we learnt at last that the remarkable increase in the fore-brain was not only one of the causes of malignancy but was to be in the end one great cause of the extinction of man. If that proved to be a fact, such a result would but class man as one of the many races of animals which perished of special over-growths and a possible lack of fertility.[9]

The degenerate or pathological character of civilization has been emphasized by many authors, including Ruskin and Carpenter. The following is from the poet Schiller

[9] *Malignancy and Evolution* (London: E. Nash & Grayson, Ltd., 1926).

(*Ueber die aesthetische Erziehung des Menschen*, 1795) and is quoted by Jung in his *Psychological Types*,[10] page 91:

I do not ignore the advantages which the present generation, regarded as a whole and measured by reason, may boast over what what was best in the bygone world; but it must enter the contest as a compact phalanx and measure itself as whole against whole. What individual modern could enter the lists, man against man, and contest the prize of manhood with an individual Athenian? Whence then arises this unfavorable individual comparison in the face of every advantage from the standpoint of the race?

The philosopher F. C. S. Schiller is even more explicit when he says:

It appears then that we can extract no guarantee of progress from the nature of man or from the nature of human institutions. There is no *law* of progress! . . . Civilization, as at present constituted, is very definitely a deteriorating agency, conducing to the degeneration of mankind. This effect of civilization is nothing new; — its discovery, however, is very recent.[11]

In the writings of Stärcke, who actually dubs the disease of civilization "metaphrenia," I find the following remarks:

Civilization seems then to be a disease which is imposed on a certain portion of society in order to obtain a certain extra gain whereby all profit. . . . Civilization from the individual point of view belongs to neurotic phenomena. . . . We see the civilization of a people or a race built up in cycles according to the mechanisms of the obsessional neurosis, until it becomes no longer bearable; then there comes about a limitation of the useful effect through the return of the repressed material in dis-

[10] New York: Harcourt, Brace & Co., 1923.

[11] *Tantalus; or, The Future of Man* (New York: E. P. Dutton & Co., 1924).

guised form, and a breaking through of forbidden things in war and revolution, according to the principles of the manic psychoses, while various "isms" analogous to the paranoid fields are not lacking. . . . Civilization demands regression, *et cetera*.[12]

Yet other considerations, to which the intelligence testers and psychoanalysts have been calling our attention for some years past, indicate that as civilization proceeds, most of us find it increasingly difficult to advance beyond, or increasingly easy to lapse into, juvenile or even infantile modes of thought, feeling, and behavior. It would seem that these tendencies must be due, at least partially, to the increasing pressure of prophylaxis and protection exerted by the controlling classes of society on all its members. We are succeeding so well in making everything foolproof for one another that even the baby termites might envy us our individual security.

The prospect is by no means pleasant. The reader will be delighted, I am sure, that I refrain from further comment.

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VIII

CARL AKELEY'S EARLY WORK AND ENVIRONMENT[1]

THE MATURE constructive activities of an unusual man whose fame becomes established during his lifetime are apt to be so widely known that they can be readily reported and appraised, but it is more difficult to evaluate the long years of struggle and preparation that necessarily precede the successful climax of such a career. This is eminently true of Carl Akeley, whose greatest achievement lay in his revolution of taxidermy, an art of obscure origin and long and gradual development in esoteric museum laboratories to which, for obvious reasons, the general public is not welcomed. The critical period in Akeley's life extended from the beginning of 1884 to the end of 1890, and as I was privileged to be his bosom friend and almost constant companion during that period, I gladly comply with Doctor Lucas' request to contribute to this memorial number of *Natural History*. And since, moreover, I happened to have kept a voluminous diary covering those years, I can precisely date most of my statements. If, in what follows, my own personality obtrudes too conspicuously, I beg the reader's indulgence for two reasons: first, because we were so intimate that I was necessarily an active, daily element in Akeley's biological and social environment, and second,

[1] *Natural History* (American Museum of Natural History, New York), vol. XXVII (1927); included here by permission of *Natural History Magazine*.

because as I peruse my diaries for the first time since they were composed with all the effusive detail of youth, my present contracted ego seems to belong to quite a different person.

I was born in 1865 in Milwaukee and lived there till I was nearly nineteen. The cerevisiacal fame which that city enjoyed in those preprohibition days unfortunately quite eclipsed the fame of its temperate and highly intellectual German population and excellent school system.

Owing to my persistently bad behavior soon after I entered the public school my father transferred me to a German academy founded by Peter Engelmann, an able pedagogue who had immigrated to the Middle West in 1848. The school had a deserved reputation for extreme severity of discipline. To have annoyed one of the burly Ph.D.'s, who acted as my instructors, as I had annoyed the demure little schoolmarms in the ward school, would probably have meant maiming for life at his hands or flaying alive by the huge Jewish director, Dr. Isidore Keller, "curled and oiled like an Assyrian bull."

After completing the courses in the academy, I attended a German normal school which somehow had come to be appended to the institution. A few weeks before my father's death in January, 1884, an incident occurred which was to influence my whole subsequent life and indirectly Carl Akeley's. Prof. H. A. Ward, proprietor of Ward's Natural Science Establishment in Rochester, New York, which was not so much a museum as a museum factory, learned that there was to be an exposition in Milwaukee in the fall of 1883 and that the local German academy, which I had attended, possessed a small museum. He decided, therefore,

to bring a collection of stuffed and skeletonized mammals, birds, and reptiles, and an attractive series of marine invertebrates to the exposition, and to persuade the city fathers to purchase the lot, combine it with the academy's collection, and thus lay the foundation for a free municipal museum of natural history. I had haunted the old academy museum since childhood and knew every specimen in it. Indeed, Dr. H. Dorner, my instructor in natural science, had often permitted me to act as his assistant. Of course, I was on hand when Professor Ward's boxes arrived, and I still remember the delightful thrill with which I gazed on the entrancing specimens that seemed to have come from some other planet. I at once volunteered to spend my nights in helping Professor Ward unpack and install the specimens, and I worked as only an enthusiastic youth can work. He seems to have been dully impressed by my industry, because he offered me a job in his establishment. I was quite carried away with the prospect of passing my days among the wonderful beasts in Rochester. Not the least of Professor Ward's attainments were his uncanny insight into human nature and his grim business and scientific acumen. He offered me the princely salary of nine dollars a week, six of which were to be deducted for board and lodging in his own house.

I entered Ward's Establishment February 7, 1884. My duties consisted in identifying, with the aid of a fair library, and listing birds and mammals. Later I was made a foreman and devoted most of my time to identifying and arranging the collections of shells, echinoderms, and sponges, and preparing catalogues and price lists of them for publication. Such is the present state of conchology that my shell-catalogue is still used by collectors. At this time Akeley entered

the establishment as a budding taxidermist, and for once Professor Ward's estimate of human nature seems to have been at fault, for as Akeley informs us in *In Brightest Africa*, he was given a salary of $3.50 a week, without board and lodging. He attached himself to William Critchley, a young and enthusiastic artisan, with the voice and physique of an Italian opera tenor, who had attained the highest proficiency in the taxidermic methods of the time, but did not seem to give promise of advancing the art. In the course of a year Akeley had more than mastered all that Critchley could teach him, and was longing for wider opportunities than could be offered by an establishment, which, after all, was neither an art school nor a scientific laboratory, but a business venture. But even so, there is reason to believe that its standards of workmanship were higher than in any of the museums that had grown up in various parts of the country.[2]

The relations between Akeley and myself soon ripened into a warm friendship. We were nearly of the same physical age, but I was the younger and more unsettled mentally, for he had been reared by sturdy parents on a quiet farm and I had been brought up in a bustling city with a superheated atmosphere of German Kultur. He was very strong and healthy, had an inexhaustible capacity for work, a great fund of quiet humor, and thoroughly manly disposition. He seemed to have been born with unusual taste and discrimination and an intuition which could dispense with mere book-learning. Of all the men I have known — and my profession has brought me into contact with a great many — he seems to me to have had the greatest range of innate ability. Al-

[2] Save in the United States National Museum.

though he later became an unusual sculptor, inventor, and explorer, he would probably have been equally successful in any other career.

In the course of time our relations settled into those of affectionate older and younger brothers. I cannot recall that we were ever even on the verge of a quarrel, and this must have been due to Akeley's self-restraint and sympathetic tolerance, because I was often irritable and unwell in those days. Owing to the fact that we did not work in the same building, our companionship was largely limited to evenings and Sundays. As I read the diaries of 1884 and 1885 I marvel at the multiplicity of our youthful interests and occupations. I cite a few passages to illustrate how we spent some of our spare hours.

MONDAY, Jan. 6, 1885. Worked on the glossary for the shell-catalogue all day. In the evening went with Carl to hear Bob Ingersoll in his lecture "Which Way?" We were much pleased with him and his wit. The lecture cleared from my mind a host of prejudices against this man who is after all a *real he man*. Weather cold.

SUNDAY, Feb. 15, 1885. Rose late. Took a walk with Carl and then went to church (Unitarian) with him to hear Doctor Mann give a magnificent sermon on the text "Out of Egypt will I call my son." Worked on algebra and read Virgil after dinner. Then walked down West Ave. with Fritz Mueller [a former schoolmate whom I was coaching in Latin for entrance to Johns Hopkins. He was the living image of the famous physiologist Johannes Mueller and probably belonged to the same family]. Tired on my return. Fritz read to me Jean Paul Friedrich Richter's "Kampaner Thal."

THURSDAY, Feb. 26, 1885. Worked on the shell-catalogue more diligently than on previous days, but am still low-spirited. In the evening read the conclusion of the Æneid and some of Zeller's "Deutsches Reich" with Louis Akeley [Carl's

brother who was attending the University of Rochester and whom I was coaching in German]. To bed at a quarter of twelve.

MONDAY, March 23, 1885. Worked all day on the foetal Marsupials: kangeroos, koalas, opossums, etc. Labelled all the foetuses and pouches. In the evening walked with Fritz and on returning read with him about 100 lines of the third book of the Æneid. The evening ended with an acrimonious dispute and I went to bed in high dudgeon.

THURSDAY, March 24, 1885. Worked all day in Prevotel's shop, changing and labelling the alcoholic fishes. In the evening attended the meeting of the Geological Section of the Rochester Academy of Sciences. Mr. Preston read to us about a quarter of Geikie's "Primer of Geology." After the meeting walked with Mr. Shelley Crump [an amateur conchologist and prosperous grocer of Pittsford, New York, to whom I had become greatly attached]. To bed at eleven.

And this is an account of a week-end with Mr. Crump:

SUNDAY, May 23, 1885. From 10 to 12 worked with Professor Ward in the shell-house, labelling Echini — the last time I saw him [for many years]. In the afternoon Mr. Crump and his friend Doctor Dunning called on me. I walked with them to Brighton and thence took the train to Pittsford. We read together some recent papers on Pasteur by Tyndall and others and then walked along the Erie Canal bank where I collected two species of Valvata.

MONDAY, May 4, 1885. Rose late. Read some of Burrough's *Wake Robin* before breakfast. Then conversed with Dr. Dunning on Shakespeare's *Sonnets* [Dr. D. was blind and with the aid of his wife was preparing a volume on the sonnets]. At 9:20 took the train for Rochester and went to work in the shell-house, finishing the family Nassidæ and part of the Volutidæ.

TUESDAY, June 23, 1885. In the morning read Bluntschli with Louis Akeley. In the afternoon went with Carl, Will Critchley,

and Mr. Crump to see the tobacconist Kimball's beautiful collection of orchids. Succeeded in making a Catasetum discharge its pollinia! In the evening read Bluntschli again after having seen Mr. Crump off on the West Shore train. Returned much fatigued. My eyes begin to pain me.

Of active, industrious young men there seem to be two types. One of them accepts a given environment and is not only satisfied with its routine and constantly recurring human contacts but prefers it to any change. These young men are apt to marry early and to become the conservative and contented *fond* of our society. Those of the other type, probably endowed with a more unstable if not more vivid imagination and with a peculiar defence reaction, or subconscious dread of being owned by people and things, soon exhaust the possibilities of their medium, like fungi that burn out their substratum, and become dissatisfied and restless till they can implant themselves in fresh conditions of growth. Akeley and I were of this latter type, and by the spring of 1885 had decided to leave the establishment at the earliest opportunity. I departed June 29 and returned to Milwaukee, but Akeley remained, apparently because the death of the elephant Jumbo, which was to be mounted for the Tufts College Museum, recently founded by Barnum, had just presented an opportunity for a new kind of taxidermic exploit. He and Critchley were put on the job, but Akeley naturally became the dominant member of the partnership and was soon absorbed in the problems of large mammal taxidermy which were to occupy him for so many years. His superb neuromuscular organization seemed to have been specially designed to give plastic expression to the refractory hide of the huge quadruped, and the successful accom-

plishment of the task furnished the inspiration for his later work in Africa, the Field Museum, and the American Museum.

Soon after my return to Milwaukee my old friend, Dr. George W. Peckham, who had long been making important contributions to arachnology and was beginning his well-known studies on the behavior of the solitary and social wasps, persuaded me to take a position as teacher of German and physiology in the high school of which he was principal. Peckham was a very learned and charming man, deeply steeped in the evolutionary literature of the time and keenly alive to the possibilities of the new morphology that had been inaugurated by Huxley in England and a host of remarkable investigators in the laboratories of the German universities. Every year he most conscientiously read, as a devout priest might read his breviary, Darwin's *Origin* and *Animals and Plants under Domestication*. We became very intimate, and I find from my diaries that for some years I regularly spent my Sunday mornings in his house drawing the palpi and epigyna of spiders to illustrate the papers which he wrote in collaboration with his equally gifted and charming wife. I was privileged to collaborate with them in one paper (on the Lyssomanæ) and to help them during the summers in their field work on the wasps at Pine Lake, Wisconsin. Under Peckham's management the biological work of the Milwaukee high school was carried far beyond that of any similar institution in the country. There were classes in embryology, with Foster as a text. We possessed a Jung microtome and the paraphernalia for staining sections and demonstrating the development of the chick, and, of course, the classes in physiology were required to master Huxley

and Martin. While at Ward's I had purchased Carnoy's *Biologie Cellulaire* and had imbibed from it an intense but rather ineffectual interest in cytology. Then most fortunately, Mr. E. P. Allis established his "Lake Laboratory" in his residence near the high school and appointed Prof. C. O. Whitman as its director and Dr. William Patten, Dr. Howard Ayres, and Mr. A. C. Eycleshymer as assistants. These gentlemen were, of course, actively spreading the gospel of the new morphology. Doctor Patten, only four years my senior and fresh from Leuckart's laboratory in Leipzig, taught me the latest embryological technique and suggested that I take up the embryology of *Blatta* and other insects. I find that I devoted nearly all my spare time to this work till 1890.

In the meantime the Milwaukee Public Museum had been established according to the plan suggested by Professor Ward, and I saw an opening for Akeley as its taxidermist. I persuaded him to come to Milwaukee and live with me. He arrived November 8, 1886, and although he was not officially appointed to the institution till November 20, 1888, he was given a certain amount of its work. We converted a barn on my mother's place into a shop and here he worked at least during the evenings for several years. I was made custodian of the museum September 19, 1887, and held the position till August 29, 1890. By that time my association with Peckham, Whitman, and Patten had converted me into a hard-boiled morphologist, and I was induced by Whitman to accept a fellowship at Clark University, where he had become professor of zoölogy a year earlier. Till October 1, 1890, when I left Milwaukee for good, Akeley and I had spent so many happy hours to-

gether that the parting was painful. After leaving the high school I had fitted up a laboratory in the house and when my eyes grew weary with the microscope I repaired to his shop and read to him while he worked or more rarely he read to me. My diary mentions the volumes we read and I wonder at Akeley's patience and apparent pleasure in listening to Bryce's *American Commonwealth*, translations of Æschylus, Max Nordau, and similar high-brow stuff. I patiently read a whole small library for at that time I had serious conscientious objections to beginning a book without reading its every word. Perhaps Akeley really heard only occasional important fragments and had found that he could carry on his own trains of inventive thought better when we were together and I was making a continual but not too disturbing noise.

After we separated in the fall of 1890 I was to see Akeley only at long intervals. I had hoped to be able to provide him at the museum with every opportunity for his work, but the city's appropriations were small, and we were unable to undertake the mounting of the elaborate groups which he was constantly building in his artistic imagination. He was able to develop his technique on a small scale, however, so that when the opportunity came some years later at the Field Museum, he had no difficulty in creating his fine groups representing the four seasons of the Virginia deer, and was fully equipped to undertake his African groups as soon as he could secure the necessary specimens and data on their habits and habitats. I feel certain, therefore, that the eight years he spent in the quiet and sympathetic Milwaukee environment where he led a secluded, abstemious life, and worked twelve to fourteen hours a day, were the most im-

portant period of his development both as a taxidermist and as a sculptor.

It appears that I was also the cause of his leaving Milwaukee. While on my way in 1893 to work in Boveri's laboratory in Würzburg, I visited the British Museum of Natural History, and was conducted through it by its director, Sir William Flower. After viewing some of the taxidermic atrocities exhibited in that Elysium of glass cases, I remarked that we had in America the most accomplished young taxidermist in existence. Most Englishmen would have dismissed this as a mere piece of Yankee boasting, but there must have been something in my voice or manner that arrested Sir William's attention, since he asked for Akeley's name and address and, as I later learned, requested him to come to London. But while he was passing through Chicago on his way to the British Museum, Akeley visited the Field Museum and was intercepted and engaged by its curator of zoölogy, Dr. D. G. Elliot.

In 1894, soon after returning to the University of Chicago where I was then instructor in embryology with Professor Whitman, I learned that Akeley was at the Field Museum. I naturally looked forward to a renewal of our old intimacy but was informed that this was impossible. It seems that Professor Elliot, whom I had never met, disliked the zoological department of the university, probably because of its strong morphological bias and the outspoken contempt of a few of its members for taxonomy, and I was naturally included as a *persona ingrata*. Moreover, he realized that he had captured a prize in Carl Akeley and was afraid that the secrets of his technique might leak out and be appropriated by some other museum. He therefore forbade any visits

and kept Akeley closely confined, and as he worked every day and far into every night, I was able to see him only once or twice during all the years I was still to remain in Chicago. Professor Elliot's procedure was not devoid of humor, because I was, of course, perfectly familiar with Akeley's methods and could have made no use of them even had I wished to do so. Many years later fate brought an ironical atonement when the National Academy of Sciences conferred on me a medal which had been established by this same Professor Elliot!

To appreciate fully the educational and æsthetic significance of Akeley's work would require a serious review of the history of taxidermy, and this unfortunately has never been made the subject of careful investigation. As a means of preserving domestic pets and the trophies of the chase the art may be ancient, but could have had little importance till extensive natural history cabinets were established in Europe during the seventeenth and eighteenth centuries. Of the first work on taxidermy, written by Réaumur [3] no copy has been found, but it may exist wholly or in part in English translation as an article in the *Philosophical Transactions of the Royal Society*.[4] I have found in the library of the American Museum a publication containing a number of Réaumur's letters,[5] in some of which, addressed to J. F. Séguier, one of his correspondents in Italy, he gives direc-

[3] *Mémoires sur la préparation des objets d'histoire naturelle* (1745).

[4] "Divers Means of Preserving from Corruption Dead Birds, Quadrupeds, Reptiles, Fishes and Insects," *Philosophical Transactions of the Royal Society 45*, 1748 (1750), pp. 304–320.

[5] Edited by G. Musset in *Annales: Société des Sciences Naturelles de la Charente-Inférieure, Académie de La Rochelle*, vol. XXI, 1884 (1885), pp. 177–258, and vol. XXII, 1885 (1886), pp. 89–191; reprinted as a volume of 183 pages in 1886.

tions for treating birds, etc., for shipment to him and describes his method of preparing them for the cabinet. The dead birds were sent packed in kegs with much salt, alum, or wine vinegar as preservatives, and his method of mounting them consisted in giving them a natural posture and then baking them in an oven till they were quite hard and dry. Another naturalist of the time, a German botanist, simply bisected his birds along the sagittal plane, spread out the two halves and pressed them like plants in his portfolios! Of course the *Dermestes* must have been delighted with collections made according to these wonderful methods, which were really processes of mummification and not taxidermy. Probably mammals, since their skins could be removed more easily than those of birds, were actually stuffed at that time.

The museum curators and their assistants throughout the greater part of the nineteenth century in France, Germany, England and the United States somehow managed to develop taxidermy to the stage in which it was vegetating when Akeley began his work. The duty of the poorly paid curator had always been to amass, hoard, name, describe, and label as many different defunct animals as possible, and the duty of his famulus the even more poorly paid taxidermist, was to impregnate them with lethal chemicals in sufficient quantity to discourage the museum pests and to try to give them a semblance of life. The result was pathetic when it was not ludicrous, because the taxidermist, at least in museums open to the public, was confronted with the stupendous problem of making dead hides thrilling to the common run of humanity, and the curator, if he was a scientist, necessarily pursued the method of all science, namely, that of abstraction, which has never been attractive to the great

majority of our species. He was mainly interested in animals in isolation from their natural environment and behavior and reduced to so much fur, feathers, horns, hoofs, bones, etc., which he could measure and describe in an esoteric jargon intelligible only to other curators in other museums. Akeley, of course, hugely enjoyed the taxidermic exhibits of those days. I remember walking with him through a certain museum and coming upon a stuffed lynx. The creature had been upholstered to about four times its volume in life, its fur had long been a happy hunting ground for *Dermestes*, and one of its glass eyes had become dislocated, so that it was wall-eyed. Just then a sunbeam stole through the dusty pane of the case and fell on that unfortunate orb. The pathetic but fiery glance which it emitted and which seemed to concentrate within itself the whole tragedy of contemporary taxidermy, threw us both into convulsions of laughter.

From the beginning, Akeley clearly realized that any animal mounted for public exhibition can have neither educational nor æsthetic value merely as a stuffed hide, furnished with a pair of glass eyes, attached to a turned wooden pedicel, and provided with a label giving its Latin and vernacular names and the name of the locality in which it was slain. He was thoroughly convinced that an animal is meaningless, except to a hard-shelled zoölogist, unless it is presented in such a manner as to convey something of its real character, or *ethos*, which is manifested by its specific motor behavior in a specific natural environment. The development of the taxidermic "group" follows naturally from such a conviction. At the present time, owing largely to Akeley's intensive study of mammalian habits and musculature and his achieve-

ments in animal sculpture and the construction of groups, no curator, in the United States at least, would dream of tolerating those indecent, not to say immoral, stuffed beasts which were lined up in the museums of the Victorian age. Furthermore, Akeley's conception was, in a sense, prophetic of a change which through the influence of the ethologists, behaviorists, physiologists and psychologists, has now pervaded the whole field of the biological sciences, so that we have come to see that an organism cannot be isolated, even conceptually, from the peculiar environment to which it has become adapted during æons of geologic time, without a serious misunderstanding of its true nature.

In conclusion I feel that I must again apologize for the large amount of autobiographical material in this article. Probably my old comrade would have pardoned this as he condoned so many of my faults. The last time I saw him, before he left for Africa, never to return, he said, "Will, I want you to go to Africa with me so that we may end our careers, as we began them, together." This remark, I believe, was neither a premonition nor an utterance of what has been called the subconscious will to death, but the expression of a desire that we might journey together to some delightful spot in the land he so ardently loved and be reunited in our old age, as we had been united in youth, by our common interest in animal life.

IX

PRESENT TENDENCIES IN BIOLOGICAL THEORY [1]

Ainsi, l'être vivant ne constitue pas une exception à la grande harmonie naturelle qui fait que les choses s'adaptent les unes aux autres; il ne rompt aucun accord; il n'est ni en contradiction ni en lutte avec les forces cosmiques générales; bien loin de là, il fait parti du concert universel des choses, et la vie de l'animal, par exemple, n'est qu'un fragment de la vie totale de l'univers. — CLAUDE BERNARD

MOST scientific investigators are, no doubt, satisfied with the course of development and optimistic in regard to the future of their own specialties. There are always theorists, however, who, in an endeavor to view particular sciences as wholes, find cause for considerable dissatisfaction with their existing condition and tendencies, and biology is most liable to become the object of such disparagement, especially during periods like the present when the adjacent physico-chemical sciences are undergoing revolutionary and brilliant transformation. Some dwell on the deplorable "Zersplitterung" and dearth of adequate theory in the life-science (e.g., Schaxel [2]). One erratic historian, Rádl,[3] finds that biology has been steadily going to the dogs

[1] Address given by invitation at a general session of the American Association for the Advancement of Science, American Museum of Natural History, New York, December 29, 1928, and published in the *Scientific Monthly*, February 1929; included here by permission of The Science Press.

[2] J. Schaxel, *Grundzüge der Theorienbildung in der Biologie*, 2d ed. (Jena: G. Fischer, 1922), pp. 1 ff.

[3] E. Rádl, *Geschichte der biologischen Theorien in der Neuzeit*, 2d ed. (Leipzig and Berlin: W. Engelmann, 1913), I, 147, 161, 270, etc.

ever since the Renaissance, though it is clear, as Whitehead has noticed, that "the biological sciences as effective schemes of thought are barely one hundred years old."[4] Another recent theorist, Bertalanffy,[5] believes that biology has about reached a stage corresponding with pre-Copernican astronomy and physics, and maintains that the biologists have not yet discovered a single law, that what they have been fondly calling laws are merely rules or generalizations. He would probably regard the "biogenetic law" as little more than an inference or conjecture. As proof of his contention he instances the frequent naming of physical laws after their discoverers and the absence of this practice among biologists, except, of course, when they mistake mere rules for laws as in the case of Mendelism. Indeed, one infers that biologists, unlike physicists, do not understand the meaning of scientific legality and would not recognize a law if they saw one. Not only have they failed to secrete the proper theoretical cohesive for their innumerable and highly heterogeneous data, but they are still addicted to the use of medieval and Aristotelian glue compounded of such antiquated ingredients as Cartesian mechanics, potentialities, dispositions, determinants, instincts, impulses, and purposes. And to increase our discomfort, we are warned by some theorists that unless we make haste and produce a few Einsteins who can furnish a sound scientific foundation for individual and social ethics, economics, politics and human genetics, our whole race will be asphyxiated under the flood of machines and other contraptions that keep pouring down on us from

[4] A. N. Whitehead, *Science and the Modern World* (New York: Macmillan, 1925), p. 141.

[5] L. Bertalanffy, *Kritische Theorien der Formbildung* (Berlin: Gebrüder Borntraeger, 1928).

the fecund womb of technology. The theorists therefore speak of the present state of biology as a crisis, but they make it look more like a mess. Perhaps we ought to study the situation.

We must ruefully admit, I believe, that biology does present an appearance of extreme confusion. This is manifestly due in part to the inconceivable intricacy of the sector of reality which the biologists have undertaken to explore and partly to the situation of this sector midway between physics and chemistry on the one hand and philosophy on the other. This situation has both its advantages and disadvantages, and one of the latter is certainly the discomfort which the investigator, who is necessarily a specialist, suffers from exposure to so many winds of doctrine, blowing from such diverse quarters. Some of these winds, laden with the heavy odors of relativity and atomic theory, now blow steadily from the domains of physics and cosmogony, while others, more intermittent, and saturated with staler effluvia, blow from the quagmires of epistemology and metaphysics, and, of course, we must not forget the odoriferous doctrinal gusts that are always rising from the more active portions of the biological field itself. How can we blame the investigator if he complains of the draughts and hurries away to the seclusion of his own specialty?

Yet the theorists may be right in censuring this behavior on the ground that the investigator needs the open air, even if it is not always very fresh. We are reminded that it is neither the observational and experimental data nor the investigational methods, but the theories that are the essential, vital constituents of a science. It would be easy to show that they cannot be dispensed with, however circumscribed

the problem of research, and it would be almost as easy to show that an abiding interest in the more comprehensive theories — those conceived in the grand manner — is all that prevents our sciences from lapsing into little more than empirical routines. I need hardly say that I am employing the word theories in this paper in a very broad sense to include also hypotheses, generalizations and fictions, as that word is used by Vaihinger in his philosophy of the "as-if," a polite substitute for the phrase "lies, damned lies and statistics." Since we seem to be justified in concentrating on the regnant theories of a science as the best expression of its tendencies during any particular period, the discussion may be largely confined to a consideration of some of the dominant theoretical interests in present-day biology.

We might divide the tendencies into three groups: those which biology in the broadest sense shares with all the other sciences, those peculiar to particular biological sciences and those that originate either in some one of the latter or in some non-biological science and tend to spread over the whole biological domain. Well-known tendencies common to all the sciences at the present time are, of course, the prodigious accumulation of empirical data and the intense specialization and differentiation of the methods and devices for penetrating reality that are demanded by the investigator's urge for more critical analysis of phenomena. The tendencies in the various biological sciences exhibit extraordinary diversity. Some of the older sciences, like taxonomy, have become largely routine accumulation of data and their discriminative refinement and systematization, while others, like genetics, are heaving with instability and display what Schaxel has called "eine theoretische Verwilderung." In

some of the sciences the births and deaths of hypotheses succeed one another with truly modern, if not always commendable speed, while the hypotheses that can maintain their vigor seek to preempt as much as possible of biological thought. Some of these hypotheses are, of course, emulative, fanciful or merely imitative, but probably none of them is altogether useless. There are also what might be called negative tendencies, such as the deficiency of historical sense, which characterizes our whole civilization and may be due, as Tilgher has suggested, to the gradual rising to control of a new social class, the proletariat, just as the eighteenth century's neglect of history was due to the rise of the bourgeoisie.

It would be impossible in the time at my disposal even to catalogue the more important theories that are peculiar to the various biological sciences, and I am surely incompetent to discuss them. All I can attempt is a rather impressionistic sketch of the theoretical conceptions that have had sufficient vitality, enterprise and generality to dominate large portions of the whole biological field. This sketch will acquire somewhat sharper contours if we begin with a list of the dozen odd biological sciences, arranged according to their more obvious affinities.

It will be granted that all these sciences have acquired considerable independence notwithstanding their complicated interrelations and that they might best be represented diagrammatically in the three dimensions of space. For my purpose, however, a simple serial arrangement will suffice, with sociology and psychology, if we insist on their inclusion, at the top and phylogenetics and taxonomy at the bottom. But any arrangement of this kind at once arouses our sense of values, if only because first and last suggest

superiority and inferiority. The sociologists and psychologists would say that this arrangement is quite acceptable, though they might differ as to which of their respective sciences should head the list. The physiologist, too, might be satisfied and contend that his science is naturally at the very heart of the whole series, or he might liken the list to a ladder extending from the earth's interior to the heavens,

Sociology
Psychology
Anthropology
Ethology
Pathology
Physiology
Genetics
Morphology
Paleontology
Biogeography
Phylogenetics
Taxonomy

FIG. 1

Taxonomy
Phylogenetics
Biogeography
Paleontology
Morphology
Genetics
Physiology
Pathology
Ethology
Anthropology
Psychology
Sociology

FIG. 2

with the sociologists and psychologists among the clouds, the taxonomists and phylogeneticists in the waters under the earth with the other blind fish, and himself alone with his feet solidly planted on the lithosphere. The dissatisfied taxonomist, however, might insist that the whole series should be inverted and likened rather to the ascending scheme of the Divine Comedy. He might say that the writhing and vociferous psychologists and sociologists should be consigned to the Inferno, that the physiologists and their neighbors,

the geneticists, belong together with their caged and vivisected animals in the Purgatorio and that the taxonomists alone abide in the opalescent effulgence of the Paradiso among the Platonic ideas and essences which from time to time deign to descend as the species and genera to incorporate themselves in the organic individuals. Fortunately, we

Sociology
Psychology
Anthropology
Ethology
Pathology
Physiology
Genetics
Morphology
Paleontology
Biogeography
Phylogenetics
Taxonomy

FIG. 3

can avoid these unhappy evaluative dissentions by simply turning our original series through an angle of 90° and likening it in its new position to a spectrum, an advancing army or a parliamentary body with a right, conservative wing (taxonomy and phylogenetics), a left, radical wing (sociology and psychology) and a Catholic center (physiology and genetics).

I shall not deny that all this seems fanciful, or even puerile, but I wish to use the horizontal list, which places the biological sciences on the same level, as a background for more serious considerations, which it seemed best to approach by first eliminating some of the superficial emotional antagonisms that appear to be fostered by our traditional hierarchical arrangement. It will be noticed that the names of one half of the sciences are printed in italic, the other half in roman type. This is not for the purpose of emphasizing another evaluative distinction, that of importance and insignificance,

but to call attention to the difference between the pure and mixed sciences. The former are often the older and are clearly the more independent in their general theoretical structure. Taxonomy, indeed, is quite independent, because it is the one biological science that has no theory, being merely diagnostics and classification. Modern taxonomy does, of course, assume a new significance in the light of evolutionary theory borrowed from phylogenetics, but it would probably have reached its present stage of development if that theory had never been thought of. Phylogenetics, morphology, physiology, psychology and sociology have clearly marked leading theoretical conceptions which are epitomized in the terms "history," "structure," "function," "mind" and "society," whereas the remaining biological sciences derive so much of their theory from their neighbors in the series, that they may be called mixed or blended sciences. Thus genetics combines morphology and physiology with an obvious though somewhat negativistic relation to taxonomy and phylogenetics; ethology, or ecology as it is usually called, mixes physiology, psychology, morphology and phylogenetics; biogeography combines taxonomy, ethology, paleontology and phylogenetics; paleontology is a mixture of taxonomy, morphology, phylogenetics and ethology. In pathology the morphology, physiology, psychology and phylogenetics of diseased organisms are combined, and anthropology may be regarded as a mixture of nearly the whole series of the biological sciences in so far as they have theoretical material applicable to man. The origin and splendid development of genetics within the past three decades suggest that other mixed sciences may arise in the future.

Each science is characterized by its theoretical constitution and this is, of course, conditioned by the peculiar interests of the investigator and the particular sector of reality to which they are directed. It is not surprising, therefore, that the sciences should be afflicted with an extraordinary number of logical oppositions, dilemmas, or contingencies between their theories. Some of the oppositions may be very persistent while others are capable of resolution or composition as soon as a higher or more synthetic theory has been excogitated to include them by a process not unlike that of the resolution of opposed statements or propositions in dialectics, on the principle that resolution is always possible between oppositions but never between contradictions.[6] Thus the formerly opposed theories of light and electricity in physics have been harmonized and the conflicts between the whole sciences of physics and chemistry are being smoothed out in recent atomic theory. A similar resolution seems to be imminent between physiology on the one hand and chemistry and physics on the other. Of course, the most acute contingency in biology is that between psychology and the other sciences and its resolution is not in sight, though some of the behaviorists are struggling to bring it about. Many less flagrant oppositions might be cited, such as that between the theory of the inheritance of acquired characters in phylogenetics and the present theories of genetics; within morphology, that between the static typology of comparative anatomy and the dynamic typology of experimental embryology; within phylogenetics, that between the monophyletic and polyphyletic theories of descent,

[6] See M. J. Adler, *Dialectic* (New York: Harcourt, Brace & Co., 1927), p. 188.

etc. In all these cases it may yet be possible to invent more comprehensive theories that will compose the contingencies and from which the opposed theories may be deduced. The whole matter of the logical contingency of theories is very complicated, as Adolph Meyer has recently shown in a valuable but rather abstruse essay.[7] I have touched on it here, because it is illustrated by the dualism between certain ideas or orientations that have an important bearing on the biological sciences as a whole.

I return to our horizontal list which I have covered with two gray, partially overlapping areas as a rough visual indication of the distribution and dominance of these two ideas in the biological parliament. It will be seen that the

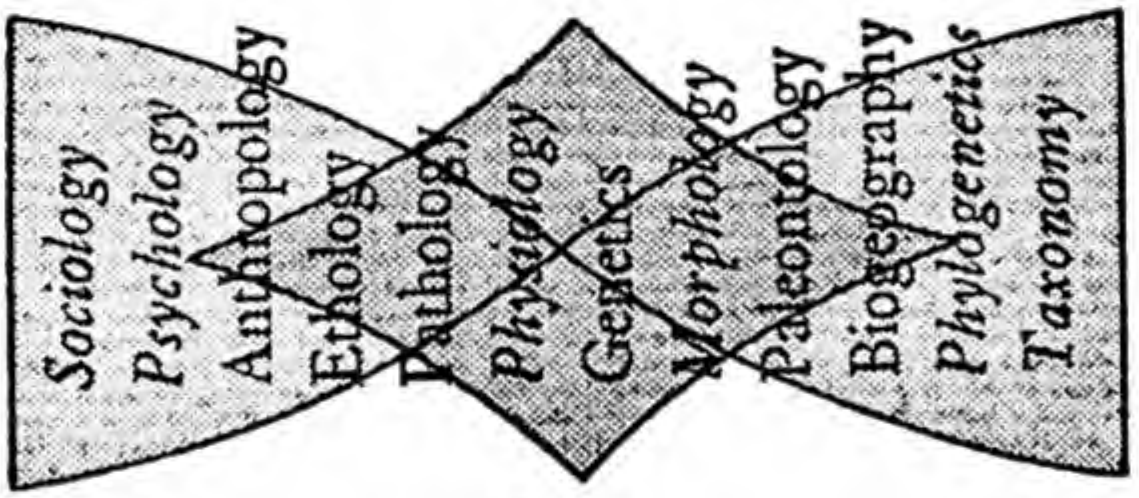

FIG. 4

darker is concentrated in physiology and the immediately adjacent sciences, experimental morphology, genetics and pathology, and rapidly dwindles away towards the two wings. It may be taken to represent theoretical preoccupation with inframicroscopic reality. The larger area is much constricted in the center and spreads rapidly towards either end and may be taken to represent preoccupation with

[7] A. Meyer, "Kontingenzerscheinungen an naturwissenschaftlichen Theorien," *Symposion* (Erlangen), I (1926), 233–268.

larger and larger microscopic and macroscopic entities. At the extreme wings these may be very comprehensive, as, for example, in taxonomy, with the whole existing fauna or flora or the whole biota considered as a unit, or in sociology with the whole Great Society, or the human race regarded *sub specie societatis*.

The same diagram may be used to illustrate the scope of two other more important contingent conceptions, or ideas, which have been emphasized by a whole school of German philosophers and historians, comprising Windelband, Troeltsch, Simmel, Rickert,[8] Münsterberg, Mehlis,[9] Kroner [10] and Meyer,[11] who designate them as the ideas of historism, or the idiographic and naturalism, or the nomothetic. To some of these writers they represent the two most significant directions of modern thought. Their differences are best illustrated by the sharp contrast between the investigative orientation of the historian and that of the physicist. The historian is interested in individuals and therefore in unique, more or less irrational, temporal manifestations of the real and in the values and purposes they assume, especially in their social and cultural relations. Since his investigations are confined to the past he is debarred from experiment and since he centers his attention on the qualitative, rather than on the quantitative, he can

[8] H. Rickert, *Die Grenzen der naturwissenschaftlichen Begriffsbildung*, 3–4 ed. (Tübingen: J. C. B. Mohr, 1921).

[9] G. Mehlis, *Lehrbuch der Geschichtsphilosophie* (Berlin: J. Springer, 1915).

[10] R. Kroner, *Zweck und Gesetz in der Biologie: Eine logische Untersuchung* (Tübingen: J. C. B. Mohr, 1913) and *Das Problem der historischen Biologie* (Berlin, 1919).

[11] A. Meyer, *Logik der Morphologie im Rahmen einer Logik der gesamten Biologie* (Berlin: J. Springer, 1926).

dispense with mathematics and employ verbal description, devoid of a special, technical vocabulary. He is more or less intuitive and may be said to be in search of meanings and symbolisms rather than causality, so that his conclusions are interpretations rather than explanations. The physicist, on the contrary, is not interested in the individual and unique, but in the general, or universal, quantitative and therefore measurable aspects of phenomena. He ignores the irrational, evaluative and purposive and confines himself to a purely rational explanation of the recurrent uniformities, or laws of reality, which he determines by analytical experiment and expresses in the exact language of mathematics. These schematic differences between the idiographic and nomothetic approaches to actuality have been greatly amplified in the works of the authors to whom I have referred and especially in Rickert's erudite treatise, which is now a classic. They have used the contrast to justify a dichotomy of the sciences into *Geisteswissenschaften* (history, sociology, psychology and philosophy) on the one hand, and *Realwissenschaften*, or natural sciences on the other. It is admitted, however, that biology is a hybrid in which both idiographic and nomothetic tendencies struggle for expression. Recently Kroner and Meyer have discussed the situation in this science, and Kroner has shown that the "historic" aspects of geology and cosmogony do not really fall within the idiographic scheme.

Although the general validity of the two contingent ideas has been questioned, especially by Tönnies,[12] Cassi-

[12] F. Tönnies, *Gemeinschaft und Gesellschaft*, 3d ed. (Berlin: Curtius, 1920); see also A. Riehl, *Zur Einführung in der Philosophie der Gegenwart*, 3d ed. (Leipzig: Teubner, 1908), p. 182; and P. Barth, *Die Philosophie der*

rer [13] and Höffding,[14] they do seem to divide the biological domain somewhat as represented in my diagram by the dark and light gray areas. Nevertheless, since biologists of all persuasions are really closer to one another than they are to the philosophers, chemists and physicists, the nomothetes have become infected with historism and the idiographers with naturalism. The language of both parties, therefore, is often so confused as to elicit from the epistemologists and theorists very disparaging remarks like those reported in the first paragraph of this paper. It is worth noting in this connection that the theory of evolution, or transformism — the only theory in the really grand manner that biology has given to the world — is obviously historistic and a creation of the idiographers. It has profoundly influenced even the philosophers and historians and is now invading atomic speculation. It is not surprising to find, therefore, that the physiologists often use such words as "organ," "function," "organism," "adaptation," "genesis," "heredity," "development," which certainly do not belong to their proper nomothetic, but to the idiographic, or historistic idiom. Of course, the physiologists might say that they use such language only when they are talking in their sl·ep or under the influence of anesthetics. The idiographers, on the other hand, feel that they have a perfect right to use nomothetic language, though they avoid it as a rule, not because it offends against

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Geschichte als Soziologie, 1. Teil, 3–4 ed. (Leipzig: Reisland, 1922), pp. 32 ff.

[13] E. Cassirer, *Substance and Function, and Einstein's Theory of Relativity*, tr. by W. C. and M. C. Swabey (Chicago and London: Open Court Publishing Co., 1923), pp. 221 ff.

[14] H. Höffding, *La Relativité Philosophique*, tr. by J. de Coussange (Paris: F. Alcan, 1924), pp. 135 ff.

good taste, but because it is too concise to serve as a medium for their own more exuberant lucubrations. It is only when a true philosopher like Hans Driesch enters the biological pasture that the idiographic sheep and nomothetic goats are made to realize the full iniquity of their contingencies. He informs the sheep that they are really woolly teleologists and the goats that they are nothing but hairy mechanists, and that he can make them all lie down and ruminate together if they will only permit him to bring in some of his queer creatures, the entelechies and psychoids, from the metaphysical barnyard to act as go-betweens. The metaphor is so distressingly bucolic that perhaps I had better try to express my meaning in less capricious language.

Driesch, Bergson, and other vitalists were keenly aware of the dilemma that had developed between our historistic and naturalistic orientations. To Bergson it wore the guise of an opposition between intuition and intelligence, while Driesch, who was in closer touch with the biological situation, was more impressed by the old conflict between teleology and mechanism. We are now convinced that both of these popular word-realisms, the one imported into biology from philosophy, the other from Cartesian physics, are little more than fetishes. While the nomothetes among the biologists were prostrating themselves before Mechanism, some of the more bolshevistic physicists very stealthily carried it off and dropped it into the sea. Most of the physicists, of course, keep mum about the matter, but occasionally one of them may be heard to berate the nomothetes who still long for their tin deity. Thus even Professor Whitehead,[15] gentlest and most courteous of mathematician-phi-

[15] *Op. cit.*, p. 23; see also Schaxel, *op. cit.*, p. 158.

losophers, after referring to the various scientific idols that have lately been stolen from their worshippers, is moved to exclaim with a touch of irritation: "What is the sense of talking about a mechanical explanation when you do not know what you mean by mechanics?" And so conservative a physicist as Professor Bridgman [16] seems to imply that any of his fellow physicists who are still tempted to cry for their old image had better hurry to the confessional. He says: "I believe many will discover in themselves a longing for mechanical explanation which has all the tenacity of original sin. The discovery of such a desire need not occasion any particular alarm, because it is easy to see how the demand for this sort of explanation has its origin in the enormous preponderance of the mechanical in our physical experience. But nevertheless, just as the old monk struggled to subdue the flesh, so must the physicist struggle to subdue this sometimes irresistible, but perfectly unjustifiable desire." Driesch's first mistake was that of accepting the mechanistic theory at its face value. Then he went astray in construing the phenomena of regulation and adaptation as manifestations of teleology, or design. He therefore set about the conciliation of the animals in the biological pasture in the wrong manner. He should have told the goats that they were miserable sinners and the sheep that they were sentimental softies and have preserved a discreet and absolute silence in regard to the entelechies and psychoids that were quietly munching ectoplasm and other spiritual fodder in the transcendental corral.

It is well known that vitalism has many more lives than

[16] P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927).

the proverbial cat and that it periodically invades and confuses biology, introducing its metaphysical entities for the purpose of resolving theoretical conflicts, just as the gods were brought down in the machine to straighten out the plots of the Greek drama. That we have again entered on a period that abhors such artifices is shown by the literature, in which references to Driesch's entelechism, the most serious and elaborate attempt ever made to provide biological *dei ex machina*, are becoming increasingly rare. The same is true of Bergson's "élan vital," which naturally played a greater rôle in philosophy. We still have on our hands, therefore, the contingency between the historistic and naturalistic ideas, and the question arises as to whether there are any means of overcoming their opposition. I believe that there are at least three recent theories, which, with some mutual adjustment, might yield a provisional synthesis, or at any rate clarify the conflict. These are the theory known as emergence, or "holism," the configuration, or "Gestalt," theory and behaviorism. The first had its origin in epistemology, the second in psychology and the third in ethology. The theory of holism, first elaborated by Alexander,[17] C. Lloyd Morgan,[18] and Smuts,[19] and recently approved by Höffding,[20] Oskar Hertwig,[21] R. B. Perry,[22] Lovejoy,[23]

[17] S. Alexander, *Space, Time, and Deity*, 2 vols. (London and New York: Macmillan, 1920).

[18] C. L. Morgan, *Emergent Evolution* (London: Williams & Norgate, 1923; New York, Holt, 1926).

[19] J. C. Smuts, *Holism and Evolution* (New York: Macmillan, 1926).

[20] *Op. cit.*, pp. 42, 160.

[21] O. Hertwig, *Das Werden der Organismen*, 3d ed. (Jena: G. Fischer, 1922).

[22] R. B. Perry, *General Theory of Value* (New York, etc.: Longmans, Green & Co., 1926).

[23] A. O. Lovejoy, "The Meaning of Emergence and its Modes," *Journal of Philosophical Studies*, II, 167–189 (London, 1927).

Bertalanffy,[24] Ritter and Bailey,[25] and others, starts from the consideration that the properties of a whole, as distinguished from a mere aggregate, sum or collection, though determined by the interrelations and interactions of the parts, are nevertheless novel and, except after previous knowledge of the mode of composition of the parts, unpredictable. That this "creative synthesis" is an empirical fact is shown by all chemical compounds and may be generalized to include all wholes, atomic, molecular, colloidal, cellular, organismal, psychical or social. There seems to be no good reason why we should not throw in also such astronomical wholes as the suns, planets, comets, solar system and galaxies for good measure, though we may have to stop this side of the universe because we can never know whether it is an emergent, even if it should prove to be a whole.

In all these existents we behold an inexplicable "social" tendency for wholes to combine and cooperate with wholes to form wholes of higher orders, or "levels," with new emergent properties. It appears to follow that at each level laws become operative which cannot be formulated for emergents of lower levels, though the converse is not necessarily true. Hence the causes, or uniform functional relationships with which the biologist *sens. str.*, the psychologist and sociologist are concerned, differ from those of the physicist and chemist, though the physicochemical laws are not annulled even in

[24] *Op. cit.*

[25] W. E. Ritter and E. W. Bailey, "The Organismal Conception, Its Place in Science and its Bearing on Philosophy," *University of California Publications in Zoology*, XXXI (1926), 307–358. For additional bibliographic references to emergence see my booklet, *Emergent Evolution and the Development of Societies* (New York: Norton, 1928). Professor Lovejoy has called my attention to an early, lucid statement on emergence in Henry Maudsley's *Body and Will* (London: K. Paul, Trench & Co., 1884), p. 132.

such singular emergents as the organisms. All this is such a commonplace that its significance was overlooked till quite recently. The novelty stressed by the writers who have expanded this commonplace of emergence into a theory of creative evolution is evidenced by the psychical shock, or feeling of surprise which we experience when confronted with the individual emergent.

The Gestaltist, or configurationist, is also dealing with wholes, but he is more interested in their peculiar irreducibility as patterns either in space or in time than in their novelty. As illustrations he points to such wholes as are represented by identical designs in different colors on a uniform background, or the same melody played in different keys. In these cases the configurations must be due to the interrelations of their component parts, because these are very different in the same configurations. Behaviorism, both in its general, ethological form and in the radical form which it has been given by Watson, is also concerned with wholes, that is, with the action-patterns of the whole organism in response to its environment. But it is not so much the novelty or the configuration of this response *per se* as its regulative and adaptive character that interests the behaviorist. No recent tendency has been so successful as behaviorism, since it has affected the attitude of investigators in nearly every one of the biological sciences. The physiologist, pathologist, psychologist, anthropologist and sociologist have all been profoundly influenced, and even students of paleontology and phylogenetics are now adopting the behaviorist's point of view.[26]

[26] See in this connection H. Karny, "Die Methoden der phylogenetischen (stammesgeschichtlichen) Forschung," in Abderhalden's *Handbuch der*

Emergence, configuration and behaviorism are so similar that we may regard them as so many partial aspects of the single conception which has been called "organicism." The individual organisms, which constitute the only materials of biology, are certainly very peculiar emergents. They may, of course, be described as spaciotemporal events or as equilibrate systems but such descriptions seem singularly depauperate to the biologist, who prefers to regard them as very highly integrated, organized wholes endowed not only with the marvelous capacities of growth, development and reproduction but also of accumulating and registering the significant results of their own adaptive experience and of that of their ancestors over enormous periods of time and of deploying at least a distorted epitome of these results in their successive generations. They are, indeed, historical beings, as Boveri [27] maintained. The investigative orientation and methods of the historistic biologist as such, therefore, required no justification.

The peculiarities of organisms are surely sufficiently extraordinary to make them the objects of an independent, unitary group of sciences, biology, but all recent researches have shown that they are not sufficient to warrant appeals to mystical "organizatory factors," "instincts," "souls" and "social minds" to account for them. In as much as the novel organic emergents are due to the interaction of their parts it is necessary to investigate these parts and their interrelations if we are to have even a partial explanation of the

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biologischen Arbeitsmethoden, Abt. 9 (Berlin: Urban & Schwarzenberg, 1925), pp. 211–500, 51 figs., and B. Dürken and H. Salfeld, *Die Phylogenese. Fragestellungen zu ihrer exakten Erforschung* (Berlin, 1921).

[27] T. Boveri, *Die Organismen als historische Wesen* (Würzburg, 1906).

whole which they constitute. Hence the nomothetic biologist is also thoroughly justified in his analytic physicochemical attitude and procedure. Regarding things as wholes, no matter how much aesthetic satisfaction or mental repose one may derive from their contemplation, is not scientific explanation. I conclude, therefore, that there is no conflict between the idiographers and the nomothetes among the biologists *sensu stricto*; they are merely working on two different levels, the physicochemical and the organismal. But organisms have also developed two other levels, those of mind and society, and it is the intrusive theories of their devotees, the psychologists, sociologists and philosophers, that disturb the harmony of the biological flock. Only those biological historists, therefore, who affect these theories, should be shorn of some of their wool to make them look more like goats. I allude, of course, to such lanuginous conceptions as "individuality," "value," "purpose" and "potentiality."

Theoretical conceptions have instrumental significance only on the phenomenal level to which they belong and become little more than word-fetishes when employed as explanations at other levels. This is obvious when philosophical, psychological and sociological notions like those just mentioned are carried down to the organismal and physicochemical levels. For example, individuality, in the sense of uniqueness, loses much of its mystery in biology since it is seen to increase at each higher emergent level as a function of the increasing multiplicity, interaction and integration of the parts in the whole, so that an atom will naturally have very little, an organism much more and a human personality a great deal of uniqueness. The irrationality and in-

definability of the individual, which have such a mesmeric effect on the historists of the Rickert school and on many philosophers, are really aspects of the emergent as a novelty. In another sense, that of temporal and spacial persistence, individuality is implied in the whole as an integration or organization of parts. Some writers, notably Roux and more recently Whitehead [28] and Höffding,[29] if I understand them correctly, regard mere persistence or survival as the fundamental peculiarity of "value," but the petering out during the past seventy years of the theory of the survival of the fittest, in which this conception of value is implicit, shows that it is far too vague or philosophical to be of any use to the biologist. This was to be expected, because the theory of the survival of the fittest, or natural selection, really came to us from two sociologists of the old evaluative school, Herbert Spencer and Malthus. Of course, the ever-increasing tendency to purge the biological sciences, including psychology and the new sociology, of all "values," does not imply that these may not be extremely significant in such purely hominstic sciences and pseudosciences as aesthetics, logic, ethics, economics, history, metaphysics and theology.

Potentiality, or possibility, is another philosophical notion that could hardly fail to be taken over by the historistic biologists when they began to study the ontogenetic development of organisms. It has persisted, under various disguises, since Aristotle — among the scholastics as the "virtues" or "occult qualities"; later as "preformation," or "evolution," in the old sense, and more recently as the "germplasm" in theories of Weismann and his school. Here the notion was elaborated in such detail that it soon revealed its true nature

[28] *Op. cit.*

[29] *Op. cit.*

— the passing off of the photograph of a problem as its explanation. When experimental embryology got under way there was still much talk of "prospective potencies," or possibilities, but the emptiness of these conceptions soon became apparent, and the words "predetermination" or "Anlagen" were substituted. In this case we again see the gradual supersession of a misplaced philosophical by a useful, though not very precise, scientific concept. One often has the impression, however, that the dormitive virtues which Molière satirized are still soporific, not only in the literature of genetics but also in that of comparative psychology where they are called "instincts." In the meantime the dialecticians inform us that the realm of possibility is the peculiar and exclusive province of the philosophers, whereas the proper province of science is actuality. If I understand Adler correctly, the realm of possibility is so inexhaustible, that the philosophers can keep up disputation within its confines till the end of space-time and then start all over again. We should, therefore, generously hand over to them all the biological possibility we can collect and felicitate them on the prospective potency of their useful occupation.

Purpose, in the sense of design, teleology, or "Zielstrebigkeit" will probably be found to be quite as sterile a conception as possibility when the biologist succeeds in clarifying the very intricate phenomena which he designates as "adaptations," "coaptations," "regulations" and "restitutions." That the notion of design is often merely a misinterpretation of emergence is suggested by Professor R. B. Perry [30] when he says: "If one speaks of the structure and composition of a whole, as the 'means,' and the peculiar synthetic properties

[30] *Op. cit.*, p. 153.

as the 'end,' one naturally supposes that the one 'seeks' the other; or exists and acts 'for the sake of' it; or that the total arrangement has been 'designed'; whereas no such thing is in the least implied." Equally finalistic and reprehensible is the statement, not infrequently encountered even in organicist writings, that the "whole determines the relations of its parts," since this, too, seems to imply — if, indeed, it implies anything — that the end determines the means. The discussion could be greatly prolonged, but perhaps I have sufficiently stressed the inefficacy of theoretical concepts when transferred from higher to lower levels where at best they merely introduce confusion. A very different picture is presented by the theoretical structures that have arisen naturally from the manipulation of the empirical data in any particular science and therefore belong, so to speak, to its own universe of discourse. I allude to such conceptions as the "species," or "taxon" in taxonomy; the "lineage," or "phylon" in phylogenetics; Waagen's "mutations" in paleontology; "age and area" in biogeography; the "type," or "character" in morphology; the "gene" in genetics; the "reflex-arc" in physiology; the "biocoenose" in ethology; the "abnormal" or "atypical" in pathology; "race" in anthropology; the "complex" in psychology and the "socius" in sociology. Most or all of these are really fictions, or "as-ifs," in Vaihinger's sense, but their practical, heuristic and synthetic usefulness is beyond dispute.[31]

Organicism, conceived as emergence, seems to me to resolve the opposition between historism and naturalism, at

[31] H. Vaihinger, *Die Philosophie des Als Ob*, 9–10 ed. (Leipzig: F. Meiner, 1927). For a discussion of the main fictions in biology see J. Schultz, *Die Grundfiktionen der Biologie* (Berlin, 1920).

least in the forms assumed by these ideas in biology. Emergence cannot offend the physiologist, because there is nothing mysterious or unscientific about it. The same is true of the configurationist's formulation of organicism, since configurations, or "Gestalten" occur also among purely physical phenomena, as Koehler has shown. And the radical behaviorists are not only outspoken holists, but adopt a decidedly hostile attitude towards many of the philosophical "residues" in biology, like those I have been considering. On the other hand, the theory of emergence must be welcome to the historistic biologist, because it releases him from the Procrustean bed of mechanism and enables him to express freely the firm conviction which he has always shared with the historians and philosophers, that evolution, both in its progressive and retrogressive manifestations, is a continual generation of novelties, an unceasing process of creation.

The upshot of this rather involved discussion would seem to be that we can clarify and tone down the oppositions among theories by rejecting a lot of adventitious and often mystical notions that have been foisted upon the biological sciences by historians and philosophers, but that certain oppositions will remain for the simple reason that organisms embrace no less than four disparate levels of emergence, the physicochemical, the organismal, the mental and the social. Hence, till the advent of a few super-Einsteins, theoretical biology must stand as a combination of oppositions — a *compositio oppositorum*. This is not a "Zersplitterung," however, as conceived by the critics, presaging decomposition or dissolution, but a sign of vigor and vital unity, like that of the healthy organism, in which what we call life is intrinsically the antagonistic synergy of its com-

ponent parts. The hopefulness of the present biological situation is even more apparent in the attitude of the creators of the biological theories, the biologists themselves. So impressive is the amount and quality of accomplishment in every one of the biological sciences that not even the narrowest specialist can now adopt the unsympathetic attitude so prevalent among eminent investigators of the past generation, for the veriest tyro soon becomes aware of his indebtedness to his fellow investigators in the most remote biological sciences. Even those tiresome old ladies, the taxonomists, are regarded as rather helpful by petulant infant geneticists, if only as donors of the India-rubber species on which they cut their milk teeth. But there is among biologists a stronger bond than that of mutual gratitude for services received and that is their common love of the living world and their desire to arouse such a love in others. The great museum in which we are meeting is one of the glorious manifestations of that feeling. There is one of Augustine's aphorisms —*res tantum cognoscitur, quantum diligitur*, a thing is understood to the degree that it is loved — which, with some interpretative manipulation, may serve to convey my meaning. Of course, Augustine would have been shocked or perhaps outraged had he deemed it possible that a mere worldly biologist could tamper with one of his most edifying remarks. From the context we may infer that the saint used "diligitur" in the sense of "contemplative or devotional love." I wish to interpret it to mean something like "investigative love." These words, unfortunately, can be construed in the sense of "perverse" or "morbid" curiosity, which might horrify or even infuriate not only Augustine but all the saints in the calendar. I hasten to state, therefore, in con-

clusion, that I am using the words "investigative love" with a strictly proper meaning and as the best I can find to designate that union of the historistic and naturalistic interests which seems to inspire an ever-increasing number of our biologists and promises the fullest ultimate understanding of animate nature.

X

HOPES IN THE BIOLOGICAL SCIENCES[1]

MY TOPIC, as worded, appears to be somewhat ambiguous. It would seem to refer either to the hopes of the general public concerning the future advantages to be derived from biological research, or to the hopes entertained by the biologists themselves in the outcome of their labors. Since I am by no means certain that the public has any definitely formulable hopes of biology, except, perhaps, those relating to relief from certain terrible diseases, I will, with your kind permission, consider only what I conceive to be the main hopes of the investigators of living organisms. Perhaps these hopes, if realizable, might be regarded as generally satisfactory. Before proceeding, however, I should like to give the subject a more definite setting.

St. Augustine informs us that when the actor who impersonated Chremes in Terence's adaptation of Menander's comedy, the "Self-tormentor," uttered the line *homo sum, et humani nil a me alienum puto* — I am a man and I deem nothing that is human foreign to me — the Roman theatre resounded with applause. Some scholars imply that it was only the actor's consummate art that elicited this response, because the sentiment must have been trite even in 163 B.C. Indeed, the line may have occurred in the lost original play

[1] Read April 23, 1931, and published in the *Proceedings of the American Philosophical Society*, vol. LXX, no. 3 (1931); included here by permission of the American Philosophical Society.

written by Menander in his youth, about 324 B.C., and the Stoic doctrine of the universal society of mankind had long been familiar. Perhaps, however, Terence's verse may have had a certain freshness, which we who live in a much more highly integrated society are unable to feel. At the present time, at any rate, we are all so interdependent that, as George Boas says, "we want to live the lives of others; we want others to live our lives. . . . The newspaper interview, publicity, the radio, the social worker, the autobiographical novel, organized play, *tours en masse*, 'just one big family,' we live the life of one of those marine animals whose nervous system is a nervenet; when one bit of it is stimulated, the whole shudders in sympathy."

The depth and universality of this feeling that nothing human is foreign to us is shown most impressively in those vast accumulations of knowledge and unverified inference which constitute what we call the humanities and the Germans call the *Geisteswissenschaften*, the sciences and pseudosciences of theology, metaphysics, epistemology, ethics, esthetics, psychology, history, sociology, economics, law, politics, linguistics and education. All of these have been ardently cultivated in the past by men who tacitly assumed that a complete knowledge of man could be secured by studying him as a unique and isolated species. Their interests were therefore exclusively or at any rate very largely *anthropocentric;* their motto that of Terence, the Protagorean "man is the measure of all things," or Pope's aphorism, "the proper study of mankind is man." To the biologist of today this interest seems to be too narrow, for the reason that our knowledge of any organism must be distorted and inadequate unless its genetic and environmental relation-

ships are given due consideration, and this obviously includes a scientific, nonvaluative, comparative study of existing man as only one of a vast series of extinct and contemporaneous organisms. Biologists are therefore *biocentric* and would be inclined to expand Terence's verse to read: "I am a living organism and I deem nothing that is living foreign to me." The Hindoos many centuries ago adopted this attitude from belief in metempsychosis; biologists have adopted it only within the past seventy years from a conviction of the truth of organic evolution. That it is not confined to biologists is shown by the increasing interest of all civilized nations in the conservation of wild-life, in the active developments of horticulture, of zoological and botanical gardens, museums, etc.

But, after all, biology deals only with a small fragment of reality — with that thin and discontinuous film of living matter which grows and proliferates on the land surfaces and in the waters of our rather diminutive planet. Poets and pantheistic philosophers, no less than geologists, chemists, physicists, and astronomers have an even more expansive, *cosmocentric* interest, and their version of Terence's line would, perhaps, read: "I am a space-time event and I deem nothing that is a space-time event foreign to me." Remote and tenuous as such an interest may seem, and without going as far as Groddeck when he says that "if one wished to utter one unquestionable truth about humanity, one would need to know the whole cosmos," we gladly admit that we still have much to learn about the biochemistry, biophysics and the cosmic significance and destiny of man.

Of course, we cannot regard the three interests, which I have mentioned, as mutually exclusive. They obviously

represent only so many natural expansions of our abiding interest in ourselves, an interest which we hope will yield a deeper, more satisfactory, and more useful knowledge of mankind in general. The biocentric certainly owe much to the older anthropocentric sciences, which have provisionally, and often erroneously, formulated essentially biological problems like those of the relations of form and function, body and mind. Certain statistical methods of handling data, and conceptions like the survival of the fittest, the struggle for existence, organic differentation as the result of division of labor, are known to have crept into the biological from the social sciences. On the other hand, we see peculiarly biological formulations, like that of the "organism," invading the theories of modern physicists, chemists and astronomers.

Now what are the hopes of the biologist? They are, no doubt, many and diverse and in part vague and inarticulate, but there seem to be two of which he is clearly conscious. He hopes, first, to obtain from investigation accurate, verifiable data that can be utilized in establishing a more adequate knowledge of the essential peculiarities of organisms, and second, he hopes to see this knowledge more extensively utilized in human thinking and behavior. He is encouraged in these hopes by his knowledge of the history of research. In the biological sciences, at any rate, this history is so recent that many of my audience have witnessed not only the development of whole sciences from casual or apparently insignificant bits of investigation, but also the profound and beneficent effects of this knowledge on the anthropocentric sciences and human behavior. In fact, the history of the dozen or more biological sciences, like that of the other natural sciences, abounds in such instances, and even the

humblest investigator hopes to initiate or to aid in initiating similar developments though he is, as a rule, quite unaware of their future possibilities. Who could have foreseen, for example, that the doctrine of organic development and evolution, that key-idea of our modern civilization, as Overstreet calls it, would arise from Lamarck's classificatory studies of animals and plants and Darwin's and Wallace's casual observations on the distribution of certain groups of tropical animals? Or who, in 1839, could have foretold that Schwann's rudimentary studies of plant-cells would culminate in our modern cytology, histology, embryology and pathology? The whole science of genetics has arisen unpredictably since the beginning of the century from two sets of observations — those of Mendel, in 1865, on the hybridization of peas — overlooked for more than thirty years, and those of my old teacher, Edouard Van Beneden, in 1883, on the chromosomal equivalence of the male and female pronuclei in the egg of the roundworm of the horse. Bacteriology and immunology can be traced back through the splendid achievements of Pasteur, Koch, Jenner and others to the almost playful observations of Leeuwenhoek in 1682 on some microbes scraped from his own teeth. Even as late as 1890, when Theobald Smith discovered the cause of Texas cattle-fever, could anyone have predicted our present knowledge of insects as carriers of pathogenic organisms, the successful completion of the Panama Canal and the sanitation of great tropical regions? Recent investigators of animal behavior like Sherrington, Pavlov, Thorndike, Yerkes, Watson and Koehler were probably aware of the general significance of their experiments, because they were performed on higher mammals and human infants, but

A. P. de Candolle, when he studied the heliotropism of plants in 1835, did not foresee the generalizations of Verworn, Loeb, Jennings and Parker, nor were such early naturalists as Réaumur, Bonnet, François and Pierre Huber aware that their careful studies of the instinctive behavior of insects would have such a direct bearing on our understanding of the human drives and appetites, as appears from the recent work of Holt, Legewie and others. Another instance, though of purely biological interest, is the development of our modern procedure in the classification of organic forms. In the more thoroughly studied groups of animals, the mammals, birds, mollusks and insects, the species of the early classifiers are now resolved into form-cycles (*Formenkreise*), or complexes of more or less variable geographical races, or subspecies. Strangely enough, the initiator of this procedure, which goes some distance towards reconciling taxonomy and genetics, was the philosopher Immanuel Kant who, in 1775, first introduced it into his account of the distribution of human races. It was independently advanced six years later by Esper in his studies on butterflies. After being overlooked for nearly a century it was resuscitated by American and German systematists and is now yielding data for a really scientific study of organic evolution and geographical distribution.

Now since the anthropocentric actually merge into the biological sciences through the group of anthropological sciences, and since, moreover, all three groups have a common subject matter so far as man is concerned, it is not surprising to find that any important biological theory rarely remains confined to the field in which it originated but promptly invades the anthropocentric group. There it be-

gins to act like a ferment, or catalyst on all our wishful thinking and time-honored, unverified assumptions and dogmas. Even if it be true that the universal unrest of our civilization is in great part due to economic conditions and the marvelous developments of technology, we must also admit that the constant increase in our biological knowledge is a very important contributory factor. Not one of the anthropocentric sciences has escaped this influence. In consequence, some of them are adopting a frankly biological orientation, others show their uneasiness in their complaints that the biologists are endeavoring to pasteurize not only the milk of humanistic knowledge, but even the milk of human kindness, and yet others are resisting the biological ferments with all the devices of a host-organism invaded by parasites. In the last group is theology, which is rapidly succumbing to the inroads, not only of the biological and physical sciences, but even of some of its former anthropocentric servant-sciences, history, anthropology and psychology. Its status in the curricula of our higher secular institutions of learning is already that of a vestigial organ, and prophets venture to predict that before the end of the century it will have no more cultural value than astrology. Metaphysics is faring no better, and is being exposed by the philosophers with slight regret, like a non-viable Spartan infant, while they busy themselves with the theory of knowledge and the religious, social and educational problems that have become acute, as the result of psychological and physiological research. The old formal logic has reached its dotage, and is yielding its place to a new non-Aristotelian, biophysical logic. Ethics is torn between the conservative old moralists, who hold fast to their supernatural sanctions and injunctions and

a radical, youthful faction insisting that moral codes shall be based on life and not life on moral codes. The social sciences, like insect larvæ, are in a stage of ecdysis, struggling to rid themselves of their valuative epidermis and to emerge as genuine biosocial sciences. Law and political science, recently estimated to be more than 2700 years behind applied natural science, unfortunately still suffer from some deep-seated disorder of the ecdysial glands, so that there seems to be no immediate prospect of a successful moult. Our traditional, academic psychology, though at present in a high fever, is undergoing a copious blood-transfusion from physiology, behaviorism, and psychoanalysis.

Did time permit, this biologizing of the anthropocentric sciences might be shown to extend also to literature and the fine arts. That it will continue is one of the fervent hopes of biologists. There is, however, a more indirect and subtle influence on humanistic thinking, through the effects on our behavior of the *applied* biological sciences, which range all the way from forestry and agronomy to eugenics. Those exerting the most powerful and salutary influence at the present time are medicine, hygiene, epidemiology and psychiatry. It seems not to have been generally noticed that all the applied biological sciences are really so many departments of biological engineering and that they are essentially only applications of ecology, since they involve scientific regulation of man's relations to his biotic, social and cosmic environments. But ecology is, in turn, rooted in physiology, neurology and behaviorism and has therefore developed a conception of man quite unlike the encomiastic, valuative conceptions of the anthropocentric sciences. However humiliating it may be, the biologist insists that we are funda-

mentally not so many indivisible, immaterial, immortal souls bombinating in the cosmos, but so many very unstable lumps of juicy colloids, largely in the form of wonderful sensory, nervous, muscular and glandular tissues, with elaborate alimentary and circulatory systems to nourish them and mineralized skeletons to hang them on and make it possible for them to act on the external world. These and some additional materials, including several meals a day, are essential to the production and operation even of a philosopher, and, as Hogben says, "when the philosopher has finished all he has to say about Nature and Life, it is the biologist who is called in by his relations to certify that he is legally dead."

Furthermore, no matter how flattering and mysterious what transpires between our ears may seem to us, our overt behavior, apart from its greater complexity, is in last analysis like that of all the other higher animals. It always consists of neuromuscular or neuroglandular responses to internal or external stimuli and of ceaseless efforts of the organism to adapt itself to its constantly changing outer world (Umwelt). That this is a basic truth physiologists, ecologists, behaviorists, psychologists and psychiatrists unanimously maintain. From this point of view, "the differences between *thinking*, *willing* and *doing* are far less significant than the identities, for all are modes of response" (Holt). Even our random movements, maladjustments, diseases and death itself are merely so many, albeit unsuccessful, reactions to stimuli.

It is not surprising, therefore, that all of us are finding it increasingly difficult to react sanely and efficiently to our extremely intricate twentieth century social and economic environments and that many of us give up the struggle and lapse either into infantile patterns of behavior or revert to

those of our troglodyte ancestors. Where, indeed, with the disintegration of traditional religion and ethics, can we hope to find the means of correcting our mental, moral and physical maladjustments, except in a biologically renovated ethics and a system of education imbued with the achievements of hygiene, psychotherapy, endocrinology and genetics?

To what extent will the biologist's hopes of a permanent influence of biological knowledge on our behavior and welfare be fulfilled? He sees all departments of chemical and physical engineering receiving an ever increasing, enthusiastic welcome from the public and a more moderate appreciation of the applications of the biological sciences which are concerned with forestry, agronomy and medicine. But those which deal with eugenics, sex-hygiene and voluntary limitation of the population encounter such a resistant barrier of emotions, prejudices and ancient mores that their general acceptance will probably be long delayed. So far as these matters are concerned, therefore, the biologist will have to possess his soul in patience. He will remember the history of the doctrine of evolution which, for very similar reasons, after more than half a century of confirmation, is still anathema to some of our institutions and the object of adverse legislation in some of our commonwealths. Indeed, the present hopes of eugenics are even less promising than were the hopes of evolution during the last decades of the nineteenth century, because evolution was mainly concerned with a reorientation of human thinking, whereas eugenics, as applied genetics, demands action. Of the eventual success of at least a part of its program, however, there would seem to be every prospect.

XI

SOME ATTRACTIONS OF THE FIELD STUDY OF ANTS[1]

AFTER more than thirty years devoted to the study of ants I find myself wondering why so very few of our nature lovers have engaged in this pursuit. I find that observing and collecting ants in many lands, quite apart from the benefit to my health, have yielded a keen delight which has remained with me and seems to have colored my recollections, so that I have acquired the habit, when regrets and unpleasant memories assail me, of effacing them with the memories of excursions in mountains, forests, and deserts peopled by colonies of thrillingly interesting ants. Certainly the pursuit of any branch of natural history may be recommended as an avocation to our youth, to convalescents, to our tired business men, or in fact to any one who craves a hobby, a surcease from the nerve-racking routine of our city life, or a valid excuse for remaining as many hours as possible in the open air of the woods and fields.[2] But no branch

[1] *Scientific Monthly*, May 1932; included here by permission of The Science Press.

[2] As the Australian myrmecologist, Mr. John Clark, remarks: "The study of ants is most interesting, and entails very little exertion. It should appeal to those whose health does not allow of vigorous work in the bush. It keeps the observer in the open, with his mind fully occupied, so that life's worries are soon forgotten, while a store of valuable information is gained. Ants are numerous everywhere. They are easily kept in artificial nests, and make interesting pets. The food required by them is always at hand, and the nests are readily made; so that no one should experience much difficulty in keeping ants for observation at home."

of natural history, in my opinion, is so well adapted to furthering these ends as myrmecology. I shall therefore discuss some of the advantages of ants as material for observation and study, and add a brief account of the simple equipment needed by the field and laboratory observer in the hope that some of my readers may be persuaded to join the ranks of the myrmecologists. In conclusion I shall call attention to a few of the many problems to the solution of which the patient and enthusiastic student of these insects may hope to contribute.

The ants, unlike many other insect families, are represented by comparatively few species in any given locality, but, being social organisms, this deficiency is compensated by the great numbers of individuals even in very circumscribed areas. The observer, therefore, always has an abundance of material at his disposal without being confused by such a maze of species, represented by comparatively few individuals, as he would encounter in many families of flies, beetles, moths, etc. The number of species, subspecies, and varieties of ants scarcely exceeds sixty in any of our New England states, and the entire ant fauna even in any one of our Southern or Southwestern states does not exceed 100 to 150. The fact that ants can be so easily collected, since their most abundant caste, the workers, are wingless and the males and females take to their wings only during the marriage flight, may also be cited as greatly facilitating their observation and study. Nevertheless, fear seems to have prevented many people from making an intimate acquaintance with these insects, though few of our North American forms can sting or bite painfully and the great majority of them are only feebly aggressive or even decidedly timid. Probably

Courtesy of Mr. W. E. Schevill

DR. WHEELER COLLECTING ANTS

Audley National Park, N.S.W., Australia

most people are so annoyed by the sensation of ants crawling over the skin that they fear to disturb colonies. This aversion, however, is soon overcome. There are, of course, certain stinging species like the agricultural ants (Pogonomyrmex) of our Western states, the driver ants (Dorylus) of Africa, the legionary ants (Eciton) of Tropical America, and the bull-dog ants (Myrmecia) of Australia, which make much greater demands on the fortitude of the observer. Excepting in such species, however, which equal or even surpass the honeybee, bumblebees, and wasps in aggressive and stinging behavior, the economy of ants, as compared with that of very many insects, is singularly open to observation. The structure of their nests, the whole personnel of their colonies, the peculiarities of the various castes of adult ants, all the different stages of the brood from egg to pupa, can be examined without any difficulty. The marriage flights of the males and females, the expeditions and wars and the founding and development of the nests and colonies, also furnish inexhaustible material for study in the field.

Ants are so extremely sensitive to the degrees of temperature and humidity of their environment and to the character of its vegetation that many species or subspecies are confined to very narrow ecological habitats. This specificity of adaptation also furnishes interesting matter for study, especially in connection with the fact that nearly all species of ants are highly variable and exhibit many subspecies or geographical races and even more numerous varieties characterized by subtle peculiarities of color, hairiness, and sculpture. Hence the great importance of the ants as material for the investigation of geographical distribution, variation, and polymorphism. An even more fascinating field for ob-

servation is afforded by the intricate relations between ants and other ants and between ants and other organisms, both animal and plant. To the former relations belong the phenomena of inquilinism, social parasitism, and slavery, to the latter the relations of ants to certain peculiar plants (myrmecophytes) and myrmecophily, or the extraordinary relations of the ants to their insect guests, parasites, and "cattle" (plant-lice, scale-insects, certain butterfly caterpillars, etc.). Field observation thus broadens out into a study of great scope and complexity and comprises many cases of the most marvelous behavior, both on the part of the ants and of their charges, known to occur among insects.

Finally a word may be added concerning the esthetic aspect of myrmecology. Ants are not usually regarded as beautiful insects, and this is probably responsible for their neglect by many entomologists. There are, however, in the tropics some very beautifully colored forms, notably in Cuba and Australia, which possess a number of gorgeously metallic green or purple species, but those of temperate regions are apt to be merely dull yellow, reddish, brown, or black. Yet even these species when viewed under a low magnification are by no means unattractive. Indeed, the form of the body, especially of the workers and males, is decidedly graceful, and the texture of the integument, with its endless variety of fine sculpturing, polish, and luster and its delicate pilosity is in many species exquisitely beautiful. Unfortunately, we possess no really artistic illustrations of ants, like the famous pictures of birds, beetles, butterflies, etc., because as yet no gifted artist has interested himself in these insects.

Since field observations on the behavior of ants can be of little value without a recognition of their various species,

subspecies, and varieties, it is necessary to collect and preserve specimens for comparative study and as essential records of observation. This taxonomic or classificatory aspect is less irksome in myrmecology than in the study of many other groups of insects for the reason, already mentioned, that the various forms occurring over a considerable area are not very numerous. The field worker will readily become acquainted with the majority of the forms in his state during a single season, but it may require several years for him to find all the rarer forms that nest in concealed situations. Often their colonies can be located only by carefully following accidentally encountered worker individuals till they return to the nest and thus betray its site. Even this method is impossible, however, in the case of certain minute subterranean forms, which rarely or never appear on the surface of the soil. These can be secured, as a rule only by using the Silvestri funnel described below.

The apparatus for field work in myrmecology is very simple and easily obtained. It comprises the following articles, all of which can be carried in the pockets of an outing jacket.

(1) Some cloth bags, each capable of containing about two or three quarts of earth.

(2) A large white handkerchief or a piece of white oilcloth about a square yard in area.

(3) A number of vials of ethyl alcohol, or if this cannot be readily obtained, of methylated or denatured alcohol.

(4) A pair of tweezers with fine, or preferably, with smooth, narrow, flattened points.

(5) A short, strong chisel, with a blade about an inch in diameter, a small trowel, or a strong-bladed plasterer's knife.

(6) A good pocket-lens. An excellent one, with magnifications of 10 and 20 diameters is supplied by the Zeiss or Leitz optical companies.

(7) A note-book.

The uses of these articles, with the exception of (1) and (2), will be obvious. An ant nest is dug up with the chisel or trowel and the earth, detritus, or dead wood quickly thrown on the piece of cloth which has been spread on the ground. The material of the nest can then be carefully broken up and the ants or any other insects associated with them collected with the tweezers before they can escape. The bags are used for holding entire small or selected portions of large nests which are to be carried home or to the laboratory for more careful inspection and collection than is usually possible in the field or for instalment in artificial nests.

To the articles listed above two pieces of apparatus, the "exhaustor" and Silvestri's modification of "Berlese's funnel," have been added very recently. They are so useful to the field myrmecologist that I here figure and describe them in greater detail. The exhaustor (Fig. 1) consists of a strong, wide-mouthed vial (A) furnished with a cork or rubber stopper (B) perforated for the accommodation of two tightly-fitting glass or metal tubes (C, D). The upper end of each of these tubes is fitted into a flexible rubber tube 12 to 18 inches long, one of which (F) ends in a perforated mouthpiece (G), the other (H) in a small glass funnel (I). Neither of these attachments, however, is really necessary. The lower end of the tube C is covered with a cap of gauze (E). If this tube is made of metal instead of glass a small piece of fine wire netting soldered to the walls of its opening

may be substituted for the gauze. In using the exhaustor for collecting ants and other small insects running over the ground, tree-trunks or foliage the vial is held in the left hand, the mouthpiece is placed between the lips and the opening of

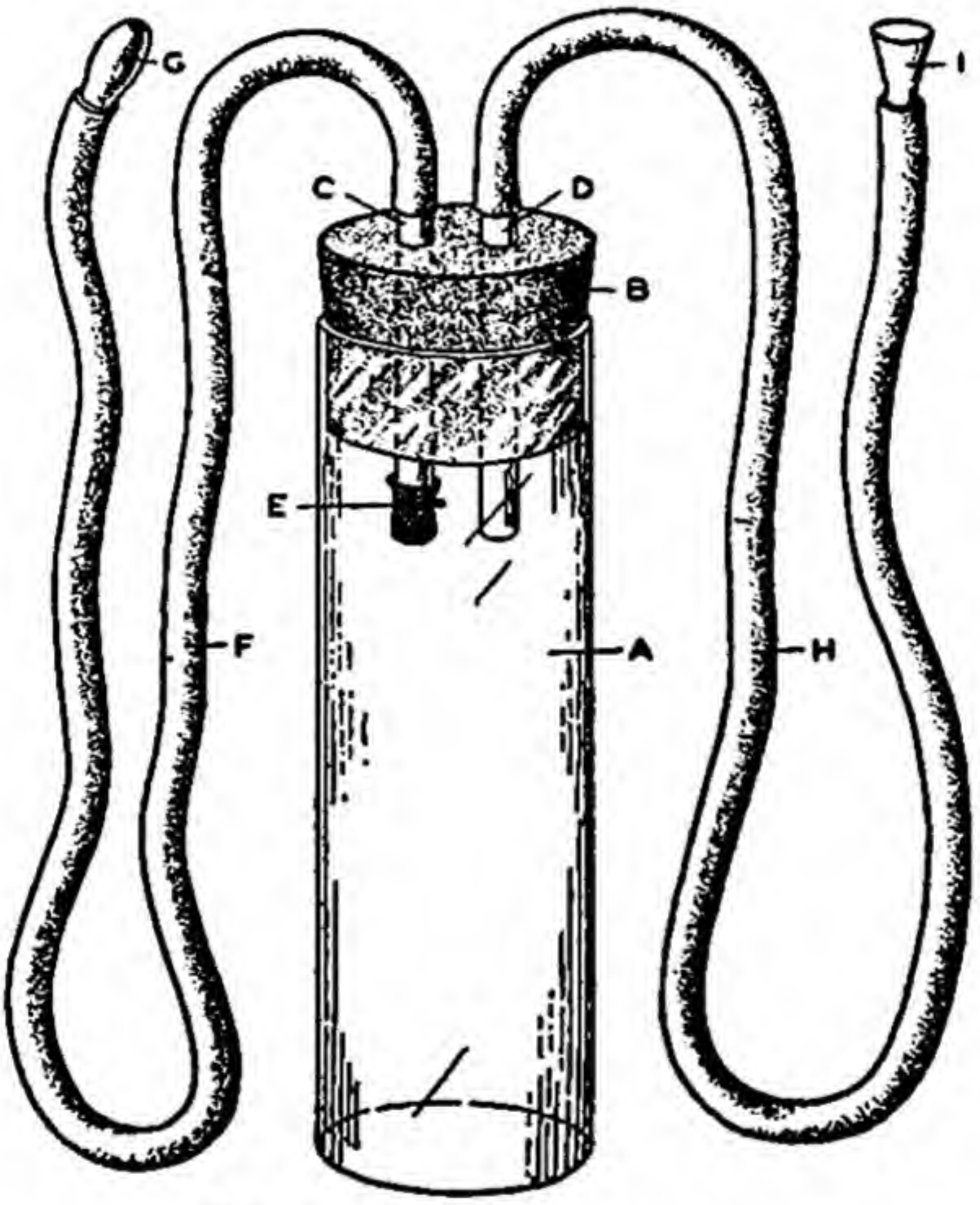

FIG. 1. THE EXHAUSTOR

the funnel or the end of tube H is placed over the insect, which can then be sucked into the vial. Large numbers of extremely delicate Arthropods, such as small beetles, Pauropods, Thysanurans, springtails, mites, gnats, ants, etc., which are apt to be more or less injured if picked up with the

tweezers or even with a brush dipped in alcohol, may thus be captured in perfect condition. If the collector carries a supply of vials of the same size as that of the exhaustor he can, after making a collection, remove it from the cork and substitute a fresh vial as often as desired. The captured insects can be either kept alive or killed and preserved by pouring a small amount of alcohol into the vial. The exhaustor seems to have been invented by the economic entomologists to avoid handling delicate insects with the tweezers or fingers. It should be of considerable use in physiological experimentation whenever it is necessary to move insects about without injuring their delicate wings or articulations or infecting them with foreign odors. The exhaustor is also very useful in transferring living ants from one artificial nest to another. A little practice with the instrument in the field will enable the collector to capture a great number of uninjured specimens in a very short time compared with the old and laborious method of picking them up one by one with the tweezers.

The Berlese funnel is an apparatus which collects minute Arthropods automatically from samples of humus or soil. In its original form it was rather cumbersome and not easily manufactured, but Silvestri has simplified its construction without impairing its efficiency. I translate his account and reproduce his figure (Fig. 2).[3]

The apparatus suggested by me was described by Mr. E. Jacobson [4] after he had used it for several years according to my direc-

[3] F. Silvestri, "Aparato para recolección de pequeños Artrópodos," *Conferencias y Reseñas Cientificas de la Sociedad Española de Historia Natural* (Madrid), 1930, pp. 10–13.

[4] E. Jacobson, "Hilfsmittel beim Fang und Präparieren von Insekten, besonders in den Tropen," *Deutsche Entomologische National-Bibliothek* (Berlin), I, 94–95, fig. 8 (1910).

tions. It is very simple, consisting of a funnel (A), which can be suspended by three cords wherever desired; of a sieve (B), with meshes one to two millimeters in diameter, and a glass vial (D) containing alcohol and attachable to the end of the

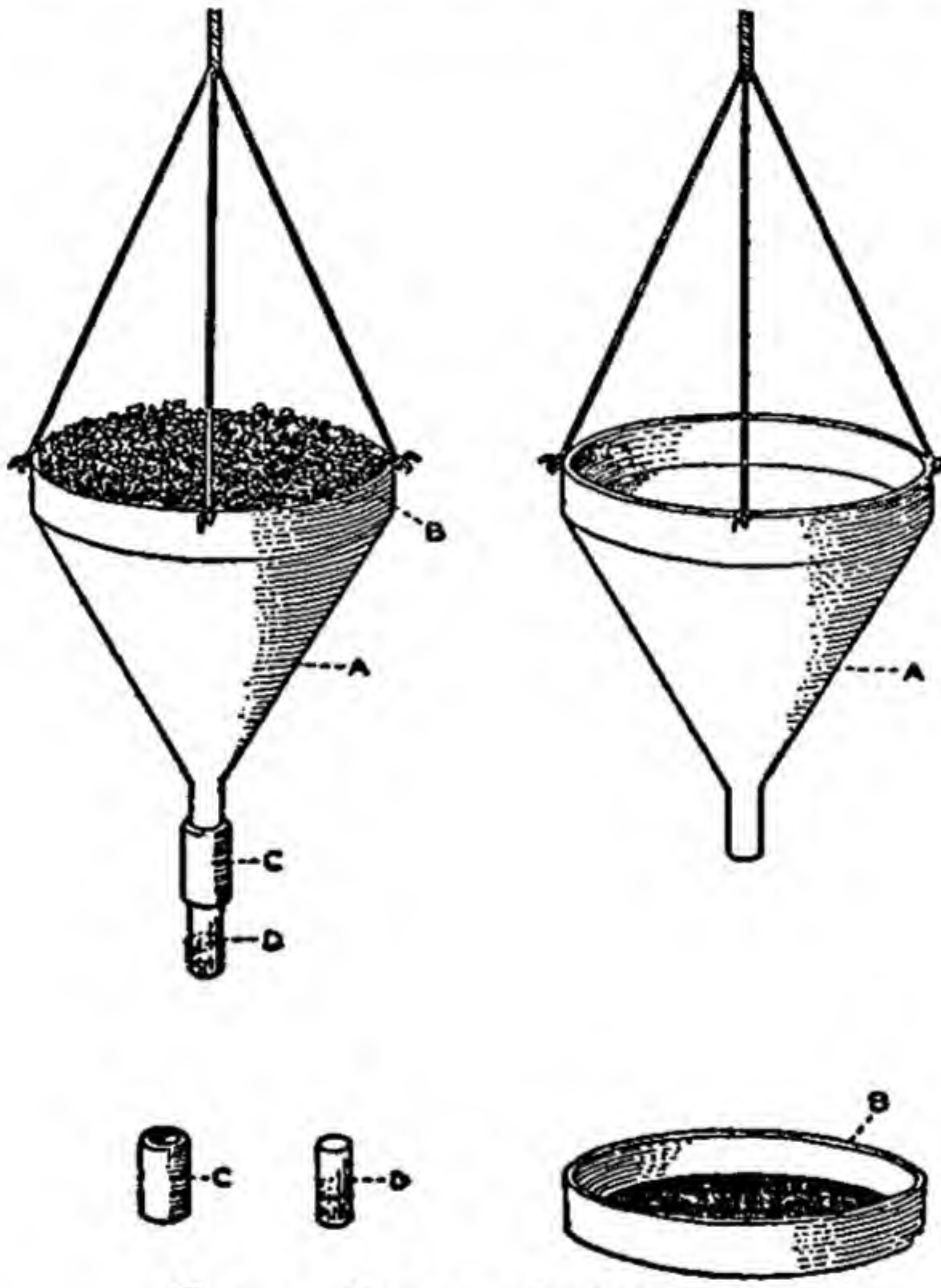

FIG. 2. SILVESTRI'S FUNNEL

funnel by means of a piece of rubber tubing (C). This apparatus, thus simplified, is very convenient, especially for traveling naturalists, because they can have two or more funnels made, of slightly different diameters, so that they can be fitted one inside the other and thus occupy but little space. In my travels I have always had with me since 1908 three of these

funnels, which have never caused me any inconvenience. . . . This apparatus may be made of sheet zinc or, more economically though less durably, of tin. It requires no attention after the selected material [humus, detritus, soil, etc.] has been placed in the sieve, [this set in the top of the funnel], and the vial of alcohol attached to the end of the funnel. The length of time the material should be left in the sieve is very variable, depending on the quantity and humidity of the material and the temperature and humidity of the surrounding air.

With the gradual drying out of the contents of the sieve, the minute insects fall one by one into the funnel and thence into the vial of alcohol at its lower end. "This simple apparatus," as Silvestri states, "should be used by all entomologists interested in the microgenton and especially by those who have an opportunity to live for some time in tropical regions where it is still possible to discover many interesting Arthropods." He coined the term "microgenton" to embrace all the minute, poorly pigmented, and usually blind Arthropods that inhabit vegetable mold, soil, dead leaves, decayed wood, etc. Quite a number of our smallest ants belong to this remarkable ecological association and are very difficult to collect without the aid of the funnel above described.

Perhaps in this connection another and very different piece of labor-saving collecting apparatus should be mentioned, namely, the automobile. Its value to the field myrmecologist especially, in enabling him to visit remote and unfrequented territory without fatigue and loss of time, cannot be exaggerated. It will probably be found that both private and museum collections have greatly increased the number and variety of their accessions, especially of insects and other invertebrates, since the automobile came into general use.

The myrmecologist will be led naturally to supplement

his field observations with observations in the laboratory. Though this may be as unpretentious as any small well-lighted room in a house, he will wish to equip it with the apparatus both of the entomologist — insect boxes, insect-pins, labels, etc., and those of the histologist — compound microscope, binocular dissecting microscope, and the usual glassware and reagents. He will also need a number af artificial nests for living colonies of ants. I have described some of the various patterns of these in an appendix to my ant book,[5] and a few others in a note published in 1910.[6] During recent years I have found that colonies of small ants can be kept for some time in ordinary Petri dishes, providing the optimum supply of humidity and food is maintained. The colonies can be readily transferred when required from fouled to clean dishes by means of the exhaustor.

The beginner in myrmecology may become disconcerted or even depressed when he discovers what an enormous mass of literature on the ants has been produced during the past two centuries. The veteran investigator may, perhaps, encourage him with the statement that we are only just entering on the more experimental and statistical and there-

[5] W. M. Wheeler, *Ants, Their Structure, Development and Behavior* (New York: Columbia University Press, 1910; reprinted, 1926).

[6] W. M. Wheeler, "Small Artificial Ant-Nests of Novel Patterns," *Psyche*, April 1910, pp. 73–75. There is an excellent chapter on artificial nests and on collecting and preserving ants in H. Donisthorpe's *British Ants*, 2d ed. (London: Routledge, 1927). The following recent literature on ant-study and artificial nests may also be consulted with profit: H. Kutter, *Züchtung von Ameisen*, in E. Abderhalden's *Handbuch der biologischen Arbeitsmethoden*, Abt. 9 (Berlin: Urban & Schwarzenburg, 1928); A. Stärcke, "Ein neues Formicarium," *Zoologischer Anzeiger* (Leipzig), XCII (1930), 152–155; H. Eidmann, *Zeitschrift für vergleichende Physiologie* (Berlin), III (1926), 776–821, and VII (1928), 39–55; E. Meyer, in Biologisches Zentralblatt (Leipzig), XL (1923), 353–404, and XLVII (1927), 264–307.

fore more serious, interesting, and enlightening study of these extraordinary insects. It is generally agreed that ants communicate with one another, but the precise character and scope of this behavior in the various species have never been satisfactorily ascertained. The same is true of the homing behavior, though it has been the subject of much valuable experimental investigation. We are still very much in the dark in regard to the feeding of the larvae by the workers and therefore of the precise method of determining the development of the various castes (polymorphism), which are such a striking feature of the ant community. Our knowledge of the parasitic and slave-making ants and of the interrelations of ants and myrmecophiles remains very sketchy. We possess no satisfactory information concerning the factors that initiate and control the movement of ant colonies to new nesting sites. The population of an adult ant-colony seems to be a true specific character, but we have almost no statistical data on the number of ants in fully developed colonies in various species, on the rate of the development of colonies, or on their life-span. Moreover, certain investigations which have been carefully pursued in Europe have never been extended to our American ant fauna, such as those relating to the interesting transportation and distribution of plant seeds (myrmecochory). This list of matters awaiting further elucidation might be indefinitely increased. For the solution of those mentioned the ant-fauna of the United States, which is so much richer than that of Europe, should afford adequate material, without resort to the splendid but less easily accessible neotropical fauna of the West Indies, Mexico, and Central America. We only need more myrmecologists.

XII

ANIMAL SOCIETIES [1]

And the least of the animals enter into their associations in peace and concord. — CLEMENT, *Epistle to the Corinthians*

Not even the communities of nations, in which everywhere class-war rages, bear the imprint of the maternal spirit, which alone is able to create a true family of the people out of the many separate members, as we are taught by the social states of the insects. — E. BERGMANN, *Erkenntnisgeist und Muttergeist.*

IT IS COMMON knowledge that many infrahuman organisms, both plant and animal, live regularly in aggregations, associations or communities more or less like our own societies. The biologists' domain is supposed to cover the entire range of this "togetherness" behavior of whole organisms from Bacteria to Anthropoid apes, while the sociologist reserves for himself the study of human societies. We may be prepared, therefore, to find considerable divergence between the biologist's and the sociologist's points of view. I infer that I am to represent the biologists in this symposium, not because of any competency to synthesize what they have learned in so vast a field, but because of my long interest in a group of animals whose activities have always seemed, even to the most casual observer, to exhibit certain interesting resemblances to the social and political

[1] Read at a symposium on Biology and Society presented on the occasion of the semi-centennial anniversary of the American Society of Naturalists, at its fifty-first annual meeting, at Harvard University, December 30, 1933; published in the *Scientific Monthly*, October 1934, and included here by permission of The Science Press.

behavior of man. Instead, however, of dwelling on these resemblances, which, though interesting, are superficial and have become rather trite, I propose, after commenting briefly on the position of the study of organismal consociations among the biological sciences and the abundance and variety of these consociations, to consider at greater length some of the more fundamental social differences between the two dominant groups of animals, represented by the social insects on the one hand and the warm-blooded Vertebrates, man included, on the other. Though my argument will compel me to trespass on the preserves of the gentlemen who are to continue the symposium, I trust that my remarks will be received as offered in a spirit less dogmatic than their necessarily brief and abrupt exposition may seem to suggest.

During the nineteenth century biology and sociology developed in rather intimate symbiosis. Though Comte founded sociology on biology, it is well known that certain important conceptions, such as the struggle for existence, the survival of the fittest, and the physiological division of labor, were derived from sociological sources and later extended to the entire world of organisms in the Darwinian theory of evolution. If we may judge from the works of Spencer, Espinas, de Lilienfeld, De Greef, Worms, Waxweiler, and others, this theory, after its first clear enunciation, seems to have been more heartily welcomed and embraced by the sociologists than by the biologists. Subsequently, however, owing to the great opportunities for investigation which had been opened up in their respective fields in the latter part of the nineteenth and the beginning of the present century, the biologists and sociologists drifted apart. The biologists specialized increasingly in the classification, mor-

phology, physiology, and genetics of the individual organism, while the sociologists seemed to lose much of their interest in biology and proceeded to ally themselves more closely with the psychologists, historians, economists, and ethnographers. It was not till the recent development of ecology as an independent formulation of what had long been known as natural history that the study of plant and animal consociations acquired scientific status. This science has now been divided into autecology and synecology, the former concerned with the external adaptations of the individual organism, the latter with the plant and animal consociations. There are reasons, however, for regarding autecology as a department of general physiology and synecology as constituting the proper domain of ecology. At any rate, synecology seems to cover the same field as sociology in its broadest sense, or what might be called general comparative sociology, which would, of course, include not only human societies but also all the various consociations of plants as well as animals. Man will always be a mammal and his basic behavior will always be mammalian behavior. That he also exhibits other and very different activities justifies the recognition of human sociology as a special field, but so many of his so-called "spiritual" idiosyncrasies are now being traced to behavioristic rudiments among the primates that the biologist will look askance at all the attempts of the ideologists to sever, or even to stretch unduly, the bonds between his science and sociology.[2]

A very brief survey will suffice to reveal the great extent and variety of consociative behavior in the animal kingdom. Indeed, there is no animal species that does not exhibit some

[2] See Note A, below.

such behavior, even if it fails to outlast the brief mating period or the temporary association of mother and offspring or amount to more than membership in some biocoenotic community. I find it convenient to classify all the animal consociations under seven heads. First, there are the loose and unstable populations known as aggregations, which consist of the same or different species and are very frequent among Protozoa, Invertebrates, and cold-blooded Vertebrates. In many of the cases recently studied by Allee and others the individuals are assembled and kept together mainly by their tropistic or sensory responses to very local environmental stimuli, but others, such as the mating congregations of many insects, the migratory swarms of locusts, etc., arise in response to interindividual stimuli or to combinations of these with environmental stimuli. We should place in a second category the very different, compact, and mainly nutritive consociations exemplified by the multicellular bodies of all Metazoan animals, the zooidal colonies of many Coelenterates, Bryozoans, Tunicates, etc., and the series of metameres constituting the bodies of Annelids, Arthropods, and Vertebrates, if, as many zoologists believe, these metameres are really serial, abortive zooids. The third category comprises the peculiar "food associations," consisting of individuals of two different species, of which one may be a plant, and exhibiting various kinds and degrees of intimacy as in the cases of predatism, parasitoidism, parasitism, commensalism, domestication, symbiosis, fungus-culture, the associations of mites, ants, and beetles with certain peculiarly specialized plants, and the vaguer phenomena of myrmecochory, mimicry, etc. The food associations are so very numerous, diverse, and economically important that they

have been made the subject of an independent science, parasitology. A fourth category comprises the flocks and herds of the birds and mammals, and includes as their most highly developed examples the troops or bands of monkeys and Anthropoid apes. A fifth category would comprise the insect societies, both temporary (subsocial) and permanent. The number and diversity of the latter are very great, since there are fully 10,000 species of social insects, each of which may be said to have its own peculiar pattern of social behavior. To the sixth category I would assign the human societies and to the seventh the biocoenoses, or what the ecologists call "communities," those consociations of animals and plants of various species, attached to particular ecological environments, such as the interdependent faunal and floral elements of a forest, cave, desert, stream, sand dune, etc. These communities are so complex, unstable, and difficult of definition that their adequate analysis seems to be impossible with our present biological methods. The totality of existing biocoenoses may be said to constitute one great super-biocoenose, embracing all living organisms, man, of course, included, and equivalent to the biosphere, or thin, more or less discontinuous film of living matter covering the lithosphere and pervading the hygrosphere of our planet.

Some authors have referred all the heterogeneous consociations I have enumerated to a single "cause," variously designated as the "social," "gregarious," or "herd instinct," but this is mere animistic verbalism. That a consociation can have no single "cause," but is determined by a set of conditions, and that each of the many consociations is determined by its own set of conditions, would seem to be inferable from the following considerations:

(1) The various consociations, as patterns of group or mass behavior, are obviously so many forms of adaptation, or "adaptates," to use a term invented by the sociologist Tarde. And while we may, perhaps, recognize homologous consociations among taxonomically closely related species, we agree with Ward and Petrucci that at least the more complex and dissimilar types represent independent, polyphyletic and therefore merely analogous, or convergent, adaptates. This seems also to be true of many similar consociations, so that no classification of the types or forms of societies can be a "natural" classification, nor coincide at all closely, except within the narrow confines of families or genera, with our morphological or taxonomic classification. I am convinced that during the long phylogenetic history of the Insecta alone very similar types of societies have arisen quite independently from the mother-offspring relation on more than thirty, and in the Aculeate suborder of Hymenoptera alone, on at least seven different occasions, but in each case more conditions than the mere mother-offspring relation must have coöperated to determine the complete societal pattern.

(2) The futility of accounting for consociative behavior by referring it to a special social instinct is shown also by the fact that every consociation is a more or less integrated, spacio-temporal system, or emergent, consisting of a number of lower-level emergents. This is clearly seen in the case of insect and vertebrate societies, which are really so many epitomizations [3] of many forms of consociative behavior like those exhibited by the multicellular individual, the ag-

[3] With the meaning of the term as employed by G. P. Conger in his *A World of Epitomizations* (Princeton University Press, 1931).

gregation and pairing of individuals, the family, with its parent-offspring relations, the food associations (predatism, parasitism, symbiosis), etc. Such components necessarily undergo great deformation, or take on quite novel aspects in the final synthesis, represented by the insect or mammalian society. In human society, of course, the creative psychological factors introduce even more extraordinary complications. In all cases, however, we are dealing with what the philosopher G. H. Mead [4] had in mind when he defined sociality as "the capacity of being several things at once."

Insect and mammalian (including human) societies have a peculiar interest because they happen to represent the highest types of behavior to which the two most important animal phyla, the Arthropoda and Chordata, have attained. Of course, the differences between these phyla are enormous, as is evident from the disparity between their members in size, structure, longevity, and behavior. I shall confine my remaining remarks to one of the most outstanding differences and one that seems to me to be of no little interest in connection with our own social organizations, namely, the high degree of integration and stability of the insect society and the extraordinarily harmonious and self-sacrificing cooperation of its individual members, as contrasted with the mobility, instability, and mutual aggressiveness so conspicuous among the members of our own society. Moralists, inventors of Utopias, and satirists have never allowed us to forget the ants and the honeybees, because they actually enjoy what is, perhaps, for us poor humans, only a social ideal.

[4] *The Philosophy of the Present* (Chicago: Open Court Publishing Co., 1932), p. 49.

Until recently the termites were not mentioned in this connection, partly because they were supposed to be ants and partly because their monumental social achievements are confined to the tropics, where the best is like the worst and uplift is unpopular.

Although it has long been known that the social insects are, as a rule, extremely aggressive towards the individuals of other communities, even of the same species, little search has been made for the conditions that have brought about the harmony between the individuals of the same community.[5] This is obviously only one of the aspects of the great stability of the social system and the result of a very long history. The numerous fossils now amassed in our museums show clearly that all the main groups of social insects had completed their social organization, their caste differentiation, and to a considerable extent also their taxonomic evolution by the beginning of the Tertiary (Eocene and Oligocene), some fifty to sixty million years ago. They must have begun their social organization, therefore, somewhere in the Cretaceous, if not earlier, perhaps as long ago as eighty or one hundred million years. Hence, if the age of our own species is put at not more than a million years, we might be tempted to condone the instability and aggressiveness of our societies as expressions of social infantilism or immaturity, but mere time and the fact that the social insects have at least thirty generations to our one can be significant only when taken in connection with the underlying behavioristic peculiarities that made for social stability or instability in the first place. The important difference lies, I believe, in what I shall call the "problem of the male,"

[5] See Note B, below.

which has been successfully solved by the social insects but not by mammal or human societies. The social insects, in fact, solved the problem by two different methods, one of which was employed by the social Aculeates (ants, bees, wasps), the other by the termites.

For obvious biological reasons the female is the social sex *par excellence*, whereas the male was originally and remains throughout the evolution of the Anthropod and Chordate phyla, except in a few fishes, amphibians, and birds, the unsocial sex. In many animals, in fact, he might more properly be called the antisocial sex. When the individuals of a species discovered in social organization a new and powerful adaptation to the environment and to one another, the male as a necessary fecundating agent could not, of course, be completely ignored, but his original constitutional differences in the two phyla resulted in corresponding differences in his social assimilability. Among the insects this is clearly seen in the exclusively female societies of the social Aculeates, all of which are really so many taxonomic families of wasps, derived without doubt from solitary wasps of the superfamilies Vespoidea and Sphecoidea. These, in turn, we regard with equal assurance as being descended from the Parasitoid or Terebrant Hymenoptera, which had evolved as early as the Jurassic, some 150,000,000 years ago (as shown by the fossil *Ephialtites jurassicus*), and are still represented in our recent fauna by thousands of species. Now throughout the Terebrant suborder and the solitary Vespoids and Sphecoids we find that the female is larger, more muscular, and generally more richly endowed than the male and exhibits an intricate behavior pattern in providing for her offspring, while the male has reduced mouthparts, less spe-

cialized antennae, a smaller, less differentiated brain, except for its optic ganglia, and a behavior pattern so meager as to amount to a mere seeking out and fecundating of the female. As if to increase his inferiority complex, the female acquired the capacity to produce viable offspring from unfertilized eggs and developed a muscular-walled spermatheca for the storage of the spermatozoa from a single mating, with glands producing a secretion to keep them alive for several weeks. When certain families of solitary wasps became social, therefore, it was easy to exclude the males from participating in the communal activities and to tolerate them about the nest only in small numbers and for a brief annual season. By enlarging the spermatheca enough sperm from the single mating could be stored and kept alive for months or even years — three or four years in the honeybee, three or four times as long in the ants — to fertilize thousands of eggs. Having solved the problem of the male by reducing him, so to speak, to an appropriated and stored convolute of sperm, the social Aculeates, long before the Tertiary, proceeded to introduce new styles of females by inhibiting the development of the ovaries in the majority of the offspring, which thus became the workers. These were still further diversified in many species of ants as soldiers, or defenders, and workers proper, or nurses and nest-builders. The division of labor thus initiated was utilized in overfeeding and thereby exaggerating the fecundity of the fertile female, or queen, and the rearing of more and more of the sterile individuals to build the nest and feed, rear, and defend the successive broods.

The termites, because of their very different phylogenetic origin, solved the problem of the male in an even more sat-

isfactory manner than the social Aculeates. They are closely related to the cockroaches, or Blattoids, and probably branched off sometime during the Mesozoic from the ancestors of the latter, the extinct Protoblattoids. Like the Blattoids the termites have a very incomplete metamorphosis, their sexes are externally very much alike, and the spermatheca of the female, which is nonparthenogenetic, is more rudimentary than in the social Aculeates and lacks spermophilous glands. These peculiarities, inherited no doubt from Protoblattoid ancestors, seem to account for the fact that the societies of the termites are bisexual instead of female, as in the social Aculeates. The termitary is founded by a male and a female, or king and queen. The king cooperates with his consort in excavating the initial chamber in the soil or dead wood and, being a long-lived insect, continues to live at her side, mating with her from time to time and thus enabling her to produce enormous numbers of viable eggs, which in some African species may be laid at the rate of 30,000 a day. The nymphs hatching from some of the eggs are fed in such a manner as to become kings and queens, which will either found new colonies or eventually take the places of the deceased royal parents of the termitary, but the great majority of nymphs become male and female soldiers or male and female workers, in approximately equal numbers, because their reproductive organs are aborted as in the exclusively female workers and soldiers of the social Aculeates. The termites therefore keep only a single fully developed monogamous male in the termitary and, as if confronted with a serious problem of male unemployment, have hit upon the happy device of sterilizing most of the nymphs of this sex in their infancy and setting them to work with

their equally sterile sisters in the kitchens, dining rooms, and nurseries and at building and defending the termitary, instead of permitting them to sit around like a lot of social parasites and annoy the females. We may say that the termites are the only animals that have succeeded in completely socializing their males.

Until recently we had little accurate knowledge of the bird and mammal flocks, herds, packs, troops, or bands, collectively designated by Espinas as "peuplades." Their organization proves to be very different from that of the social insects, because the individuals among the higher Vertebrates are much more highly differentiated than they are among the insects and other Invertebrates. The roles of the sexes, too, are more specialized. This is especially true of the male, which in the higher Vertebrates is usually larger, stronger, more restless, more inquisitive, more exhibitionistic, bolder, more reckless, more brutal, more pugnacious, and less sagacious than the female. He eventually becomes, therefore, a much more serious social problem than he is among the insects. Indeed, he acts like a violent ferment in Vertebrate group life, increasing both its constructive and destructive mobility and accentuating its dynamically stratified organization.

We may select as a paradigm of Vertebrate Society Schjelderup-Ebbe's account of a flock of domestic fowl.[6] Close observation shows that every bird in the flock is a personality, determined by its ambivalent dominance-sub-

[6] T. Schjelderup-Ebbe, "Die Despotie im sozialen Leben der Vögel," *Arbeiten zur biologischen Grundlegung der Soziologie*, 2 vols. (Leipzig: Hirschfeld, 1931).

mission reactions in relation to every other bird. Thus bird A can peck bird B and B can peck C, etc. An alien fowl may enter the flock but acquires a definite status or relationship to the other birds only after demonstrating its abilities as a pecker and the extent to which it is resigned to being a peckee. Schjelderup-Ebbe calls the hierarchy of status, which I have briefly described, the "pecking order." There may be one individual, the "α-bird," usually but not necessarily a mature cock, which has the right to peck every other bird, but there is obviously no ω-bird, unless we apply the term to a dead bird. The pecking order, however, is far from being a fixed and constant hierarchy. It is really very complicated, because the interrelations of the birds are often triangular or polygonal, A pecking B and B, C, but C may be able to peck A, or the series may be ABCDA with a resolution of the rectangle into triangles, ABCA, BCDB, etc. Furthermore, the order is constantly changing with the changing physiological state, or age, vigor, and health of the individual birds. The diseased, disabled, and aged soon descend through the ranks of peckees, while the young, after a long submissive role, promote themselves as rapidly and as far as their strength and pugnacity will permit to the ranks of peckers. Schjelderup-Ebbe noticed that the higher the birds stood in the pecking order, the better their general health and the more self-confident their behavior seemed to be, while those of the lowest rank wore a dejected and bedraggled appearance.

The organization of the herds and packs of the lower mammals is like that of the bird flock, except that we should have to call it a biting instead of a pecking order, or with recent students of mammalian behavior a "scale, or order of

dominance." As long ago as 1892, Hudson in his *Naturalist of La Plata*, writing of the packs of semiferal dogs kept on the cattle-breeding establishments in Argentina, remarked that "from the foremost in strength down to the weakest there is a gradation of authority; each one knows just how far he can go, which companion he can bully when in a bad temper and wishes to assert himself and to whom he must humbly yield in turn." A dominance scale of fundamentally the same type has been observed in the troops of various monkeys and anthropoid apes but is, of course, more elaborate, as we should expect from the greater physical and psychical plasticity of these creatures. It is scarcely necessary to emphasize the fact that in man the very ancient mammalian dominance scale has not only persisted, but has become even more highly differentiated than in our primate ancestors. We are all born into such an order, the family, and all our institutions — governments, armies, navies, schools, churches, business-houses, factories, etc. — are so many magnificent pecking orders, which condition and regulate our lives and keep our emotions oscillating between elation and misery, according to the position we happen to be holding within them.[7]

In order to bring my argument to a conclusion something more must be said about the mammalian male. When mature he naturally occupies a higher rank than the female in the dominance order of the group. Owing, however, to the decidedly unsocial character of his behavior, which manifests itself almost exclusively in voracity, pairing, and fighting with other males, he is always, so to speak, socially more or less indigestible. There seems to be no reliable rec-

[7] See Note C, below.

ord, at least among the lower mammals, of a male providing food for the female or young or even protecting them. Indeed, after pairing, the sexes seem to become indifferent or even hostile to each other and the female retires to bear, suckle, and rear her young in a safe lair or retreat which she alone establishes. She thus forms a family with her young of both sexes and in advanced life may become the leader of a herd consisting of several such female-offspring families (ruminants, elephants, cetaceans, etc.). Here the social organization unmistakably resembles that of the social Hymenoptera, since the male is not a member of the family.[8] Even in mammals as high in the scale as bats the two sexes form separate peuplades. Such social ties as the males of these and other mammals exhibit among themselves may be due to social conditioning while they are still young and under the tutelage of their mothers. In the seals and more conspicuously among the apes, as shown by Zuckerman's observations on the troops of baboons[9] and the scattered published accounts of the anthropoids, the adult males are found definitively installed within the group and giving full expression to their dominance.[10] In the troops of baboons each of the mature males, the "pashas" or "overlords," secures as many mature females as possible to form a harem, which he carefully guards and to the outskirts of which the younger and less dominant bachelors attach themselves. As soon as the pasha's vigor declines, they snatch away the females and set up as pashas on their own account. A troop

[8] See Note D, below.

[9] S. Zuckerman, *The Social Life of Monkeys and Apes* (New York: Harcourt, Brace & Co., 1932). For data on the anthropoids see R. M. and A. W. Yerkes, *The Great Apes* (Yale University Press, 1929).

[10] See Note E, below.

of baboons is, therefore, far from being an urbane and amiable society. The unsocial character of the male reveals itself even more clearly, both among the lower mammals and the Anthropoid apes, when he becomes senescent and impotent and wanders away from the troop or herd to lead the life of an anchorite "rogue." The female, on the contrary, as a virago, acquires a certain male dominance and becomes the matriarch of the herd without serious loss of her social interests.

At first sight human society seems to have solved the problem of the male. At any rate, the reader of many sociological treatises is left with the impression that human groups are uniformly bisexually socialized throughout. Certainly the majority of men are far more social than the male apes. We have, unfortunately, no knowledge concerning the origin of the human species or of the social role of the sexes in its earliest groups. Although authorities agree that none of our extant anthropoids can be in the direct line of man's descent, there is considerable difference of opinion in regard to the point of divergence of his immediate ancestors from the hypothetical primate stock. Some believe the divergence to have taken place as early as the Oligocene, others not till the Miocene, and then from some common ancestor of the chimpanzee and gorilla, while at least one author regards man as a polyphyletic species, derived from several hypothetical primates, each of which gave rise to one of the extant species of anthropoids. Although the character of man's earliest social organization is unknown, the researches of ethnographers, archeologists, and historians show that it was in all probability what it still is, a dominance order, or what Sorokin calls a "social stratification," resembling that

of the birds and mammals.[11] The great physical energy and unequal endowment of the individuals within this order, and especially the predominance of the males, evidently account for the extraordinary restlessness and mobility of human societies.[12] Even in primitive human societies there must have been far more coöperation between the sexes than there is among the higher mammals. This coöperation may have had its origin, as Zuckerman suggests, in a division of labor between the sexes at the time when man changed from a vegetarian to a largely carnivorous diet,[13] but it seems to me that the pronounced socialization of the male must have been due in great measure to the intensive social conditioning to which he was subjected by the mother and the other members of the family during his infancy and childhood, which are so much longer than in other primates. One is tempted also to look on the matriarchal, or matrilineal type of human society, which, according to many ethnographers, was once universal and still survives among many peoples, as eminently suited to socializing the male. Even in these societies there is a clear division of labor between the sexes, since the males do the heavy work, hunting and fighting, and also function as chiefs and shamans.[14]

Be this as it may, however, the male has now become so dominant in our modern patriarchal societies that we may regard them as male societies in contradistinction to the female societies of the Hymenoptera and lower mammals

[11] P. Sorokin, *Social Mobility* (New York: Harper & Brothers, 1927). See Note F, below.

[12] See Note G, below.

[13] See Note H, below.

[14] Cf. R. Briffault, *The Mothers*, 3 vols. (New York: Macmillan, 1927; abridged ed., 1931) and J. H. Ronhaar, *Woman in Primitive Motherright Societies* (Groningen, The Hague: J. B. Wolters; London: D. Nutt, 1931).

and the bisexual societies of the termites. Furthermore, the manifestations of this dominance show clearly that the human male has never been adequately socialized. Throughout the ages the aggressive, emotional instability, intense egoism and pugnacity, not to mention other unsocial and antisocial tendencies inherited from his Anthropoid ancestors, have kept society in constant turmoil, so that human history is little more than an interminable record by sober and impressionable males of the abominable behavior of other males. We might, perhaps, divide the members of this sex very roughly into three classes. One of these, the majority, comprises the completely socialized individuals who, in collaboration with the women, maintain the social structure. The second class is very small and comprises less socialized individuals whose dominance is manifested mainly in the intellectual and emotional fields. These males really constitute two ill-defined subclasses, one of which may be said to create the great cultural values (sciences, arts, technologies), the other the great cultural illusions (philosophies, theologies, social utopias). To the third class we may assign a not inconsiderable number of criminals, or individuals of low mental age and with unbalanced endocrines, who in the past have succeeded in wrecking every great civilization. We have all been witnessing recently such an extraordinary display of antisocial behavior by males of this class in continental Europe, the Orient, Cuba and the United States, that further comment is unnecessary.

After I had written the first draft of this paper, I was pleasantly surprised to find that I must have been in somewhat belated telepathic rapport with Professor Ernst Bergmann, of the University of Leipzig, who very recently

developed essentially the same thesis, with much greater eloquence and erudition, in a fascinating book entitled *Erkenntnisgeist und Muttergeist*. He has, in fact, constructed a grandiose "sociosophy" of the sexes out of materials drawn from the remotest by-ways of religion, ethics, philosophy, history, and biology.[15] I should differ with him, perhaps, in placing more emphasis on the fact that all progress in our civilized societies is initiated by a relatively small portion of the male population, whose restlessly questing intellects are really driven by the unsocial dominance impulses of their male mammalian constitution and not by any intense desire to improve society. Female societies, like those of the Hymenoptera and lower mammals, and bisexual societies, like those of the termites, are indeed peaceful and harmonious, but also stationary and incapable of further social evolution.[16] Even the matriarchal clans of primitive man advanced towards civilization only after they had become patriarchal.[17] We seem to be confronted with the trilemma of either finding some means of socializing our males more completely, or of returning to a more unprogressive bisexual society like that of the termites (Russia already shows a suspicious approach to such a society), or of lapsing into something like Spengler's Fellahin society. For thousands of years attempts have been made to socialize the unsocial and antisocial males by fasting, prayer, sermonizing, systems of ethics, idealistic philosophies, legislation, prohibition, punishment, and discipline, but with very indifferent success. It is always in order, of course, to

[15] E. Bergmann, *Erkenntnisgeist und Muttergeist*, 2d ed. (Breslau: Hirt, 1933). See Note I, below.

[16] See Note J, below.

[17] See Note K, below.

suggest a thoroughly reorganized mental and physical education of the young as a cure for our social ills,[18] but it is equally probable, as Bergmann insists, that only an adequate knowledge of the biology and psychology of the sexes will enable us to solve the problem of the male. Fortunately, the youthful sciences of endocrinology, genetics, eugenics, penology, and psychiatry are beginning to provide us not only with this knowledge but also with suggestions for its practical application.

NOTES

A. Certainly the fact that there are many weaknesses in the organicist analyses of the earlier sociologists, as Keller, Ferrière, Sorokin and others have shown, is no excuse for the modern sociologist's lack of interest in the animal consociations. Recent investigations, some of which are briefly considered in this article, are showing with increasing clearness that the sociologist can still derive valuable suggestions from infrahuman group phenomena. Not only does individual animal behavior prove to be much more subtle than the earlier zoologists supposed, but animal groups exhibit many activities that are very difficult or impossible of analysis in human societies. The animal groups are not only more numerous and more diverse and therefore more richly illustrative of many patterns of social behavior, but also more sharply delimited in space and time than primitive human societies. The animal consociations also possess other methodological advantages, since they can be isolated, their personnel controlled at will and their behavior subjected to ex-

[18] For an interesting account of the proposals of philosophers and educators see Will Durant, *Philosophy and the Social Problem* (New York: Macmillan, 1917). A very suggestive approach to the philosophical biologists' view of ethics is given in Trigant Burrow's "Crime and the Social Reaction of Right and Wrong," *Journal of Criminal Law and Criminology* (Chicago), XXIV (1933), 685–699.

perimental investigation. Their shorter life-span, moreover, enables us to study their origin and growth, their pathology and eventual extinction.

The extent to which Sorokin would allow the sociologist to adopt an organicist view of human society is indicated in the following quotation (*Contemporary Sociological Theories*, New York: Harper, 1928, p. 207): "In bio-organismic theories we must strongly discriminate between two different classes of statements. The first class is composed of the statements that sociology has to be based on biology; that the principles of biology are to be taken into consideration in an interpretation of social phenomena; that human society is not entirely an artificial creation; that it represents a kind of living unity different from a mere sum of the isolated individuals. These principles could scarcely be questioned. They are valid. They are shared, however, not only by the bio-organismic school, but by a great many other sociological schools. In this sense they do not compose a monopoly of the bio-organismic theories, or their specific characteristics."

B. Exceptions to the latter statement are the slaughter of the drones and the battles between old and young queens in the honeybees, the slaughter of the soldiers by the ant *Pheidole militicida* and apparently also by some termites, the destruction of their own brood by wasps and ants, the devouring of portions of their young brood by colony-founding queen ants and the assassination of the nest queen by her own workers and the adoption of a parasitic queen in her place by some host ants. All these cases, except the last, are motivated by the failure of the food supply at certain times or seasons and are really attempts to preserve the life of the colony. The last case, according to Forel, is due to the preference of the workers for a small, young and very fecund queen instead of their own large mother, because the latter demands more food. Perhaps, however, other attractions of the parasitic queen, such as agreeable secretions, which, like those of some myrmecophilous beetles, tend to pervert the appetites of the workers, may be the true reason for

adoption. We are, nevertheless, dealing with a distinctly pathological condition.

C. Notwithstanding the development of dominance in man it seems only occasionally to have attracted the attention of sociologists and psychologists till recently. In social psychologies it was often briefly treated as the "instinct of self-assertion" till those *enfants terribles*, the psychoanalysts, and especially Adler, began to rear an imposing doctrine upon it. Now we are all familiar with the exaggerated or pathological manifestations of dominance — the superiority complex, self-maximation, the regal self, the Jehovah-complex, the Messianic complex, the God-complex, the masculine protest, exhibitionism, sadism, etc. Its more temperate, normal aspects certainly did not escape some of the philosophers, such as Hobbes, Schopenhauer, Nietzsche, Hocking, and Spengler, but their pet term — "the desire for power" or "the will to power" — and those of the moralists, psychologists and historians — egotism, egocentricity, self-interest, ambition, emulation, competition, elation, aggressiveness, greed, pride, vanity, display, authority, prestige, coercion, supremacy, dominion, tyranny, conquest, oppression, sovereignty, despotism, militancy, etc. — are so familiar that they lack the thrill of those fine psychoanalytical terms. Several social psychologists — McDougall, Tansley and others — regard dominance or self-assertion as an "instinct" and contrast with it another "instinct," "self-abasement," variously designated also as submission, subjection, subordination, allegiance, subservience, obedience, compliance, inferiority complex and masochism. If we regard dominance as an instinct it is certainly one so primitive and fundamental as to characterize all living substance and to be equivalent to self-preservation. Seneca said "*vivere militare est*," which is paraphrased by Ortega y Gasset when he defines life as "the struggle, the effort to be itself." Adler admits that "the will to self-determination in the narrower sense, *i.e.*, to power, is a mental factor which . . . derives from far down in the animal world." Spengler, in his *Man and Technics*, expresses the same thought more explicitly when he says: "The free-

moving life of the animal is struggle, and nothing but struggle, and it is the tactics of its living, its superiority or inferiority in face of 'the other' (whether that 'other' be animate or inanimate Nature), which decides the history of this life, which settles whether its fate is to suffer the history of others or to be itself their history. *Technics is the tactics of living*; it is the inner form of which the procedure of conflict — the conflict that is identical with Life itself — is the outward expression." Curiously enough, Ellwood (*Sociology in its Psychological Aspects*, New York and London: Appleton, 1912, p. 228) regards the "instincts" of self-assertion and self-abasement as "peculiarly human." At this point we naturally ask whether it is possible to distinguish at all clearly between self-preservation of a living and that of any stable physicochemical system. Whereupon the consideration that mere existence necessarily implies some degree of self-preservation or self-maintenance at once lands us in the cactus-thickets of philosophy. We all at times experience the "pure-cussedness" of inorganic bodies and feel a dim mental affinity with the animistic savage. And what is "instinct," as employed in most biological and psychological literature, but camouflaged animism? Others are very doubtful whether there is a special instinct of self-abasement. "From the beginning," says A. H. B. Allen (*Pleasure and Instinct*, New York: Harcourt, Brace, 1930), "every living thing has only existed by asserting itself and refusing to give way to others," and submission is imposed by the superior force of the dominant organism. "It is always possible to yield in a combat; and the yielding can hardly be called a separate instinct." "Submission is nothing but the negative of self-assertion; it is the giving up of self-assertion, accompanied by the opposite feeling, that of pride negated or taken down." Moreover, it has not been demonstrated that self-absement is innate, though in its pathological form, as masochism, it is accompanied by a positive feeling of pleasure. Other emotions such as fear may also accompany submission. C. R. Carpenter, one of the younger behaviorists, regards both dominance and submission in the black howler monkeys of Panama as merely positively and negatively condi-

tioned reflexes and hence as learned reactions, or habits. That this opinion may be correct is indicated by the phenomena of domestication, which depends on man's dominance and the animal's submissiveness. In some of our domestic animals this submissiveness has to be secured by renewed "training" in each generation, in others it may require only a slight reconditioning of the animal's juvenile behavior.

D. A few writers (*e.g.*, Jennings) do not regard the mother-offspring group among ants, bees, etc., as a family, probably because it contains no father. They seem to understand the term only in the derived juristic sense of the Latin *familia*, which stresses the presence of the male progenitor (*pater-familias*) who is not like the mother "*naturâ vera et certa*," but "*jure verus et certus*," that is, a legal fiction. Originally the term *familia* meant the servant body of a household and has since acquired numerous meanings (see *Century Dictionary*). Since a widow and her children are called a family, objection to using the same term for the mother-offspring group of the social insects and lower mammals would seem to be mere quibbling.

E. Male dominance may have many different expressions. Even the voice may become an implement of this urge. The vertebrate male must have discovered long ago that his voice was more powerful and terrifying than that of the female and that he could often employ it effectively without endangering his hide. His voice proved to be particularly useful when his possessions were disputed by other members of his group. This is seen in many Vertebrates ranging all the way from the growling dog guarding his bone, the male song-bird preempting his breeding territory and the male howler monkey preempting the feeding area of his troop to the dogmatic, hortatory theologian defending his knowledge of the supernatural and the vociferous political orator defending his equally fictitious knowledge of economics and state craft.

Another interesting aspect of dominance in an extreme and sadistic form is exhibited in the courtship and mating behavior

of animals. Major R. W. G. Hingston has made much of this aspect, which he calls "hostility" in his recent book, *The Meaning of Animal Colour and Adornment* (London: E. Arnold, 1933). I quote a few paragraphs (pp. 325–326) from the conclusion of his thirteenth chapter: "The sex act is a double act. It consists first in an act of male rivalry, second an act of sexual union. The two are interlinked and the second is dependent on the first. In all animals there occurs a preliminary rivalry — by physical battle or threatening gesture or vocal utterance — before actual union is fulfilled. Often this rivalry is fierce and continuous. Several species have special assembling-grounds where the males come together for the sole purpose of developing this first stage in the act. The usual view of all this fighting is that it takes place for the possession of the females or the holding of breeding territories, and these undoubtedly are manifest results of it. But I am confident that, apart from these results, it fulfils a biological necessity of far deeper and wider significance. Is it likely that male animals would keep special arenas for the purpose of going through elaborate gesticulations or would indulge in long singing-contests unless their emotional natures demanded that there must be an outlet for their developing passions?

"All this rivalry then is of deep importance. And its importance, I believe, lies in the fact that it brings to full development that hostile emotion which is the first step in the act of coitus. This hostility is directed to the rival male; nevertheless, it is a fundamental step in the development of capacity for fertile union with the female. Rivalry does not occur just because a rival is present; I believe it must occur if full sexual activity is to develop. Rivalry and coitus are biologically interdependent; the one must be developed and brought to perfection in order that the other may be fully efficient. Indeed, I regard the act of coitus as the final step in the act of battle. It is, as it were, a demonstration to the male of the final achievement of his hostile intentions, satisfying all that he has battled for so intensively and standing in his emotional nature for the defeat and annihilation of his rivals. The sex act is then not a mere male-female

contact, but rather an act of fierce hostility directed for a time against rivals of the same sex and receiving complete fulfilment through an act of union with the opposite sex. But fundamentally and all through the sex act has a hostile content.

"This view will later throw light on so-called courtship behaviour and on the important problem of sterility. Also it will help us to understand why the generative organs have this dual function. The testes not only secret the sperm, but also control the fighting machinery. Why should these two functions be allocated to one organ unless the two functions were closely interknit in the fulfilment of the generative act? And on our view they are interknit, in that the efficiency of the sperm-producing function depends on the fulfilment of the act of battle."

F. Sorokin says: "Any organized special group is always a stratified social body. There has not been and there does not exist any permanent social group which is 'flat' and in which all members are equal. Unstratified society, with a real equality of its members, is a myth which has never been realized in the history of mankind. This statement may sound somewhat paradoxical and yet it is accurate. The forms and proportions of stratification vary, but its essence is permanent, as far as any more or less permanent and organized group is concerned. This is true not only of human society, but even in plant and animal communities." The term "stratification" is unfortunate, perhaps, in that it suggests a rigid or static order. Professor Sorokin, of course, makes it abundantly clear in his book that he is dealing with a dynamic organization.

G. The greater physical energy of human individuals compared with those of other animal species is significant. As Pitkin says: "Man has built up, through the ages, a huge fund of physical energy with which to maintain himself against the hostile forces of his environment. He possesses more than three times as much, for each pound of his body weight, as any other mammal which has yet been measured. For each pound of flesh in a horse, cow, dog or cat, there is considerably less driving power through adult years than there is in five ounces

of human flesh. (The exact ratio seems to be 2: 7.75, according to Rubner.)"

H. Zuckerman, *The Social Life of Monkeys and Apes*, p. 316: "At its lowest level, according to most authorities, the family of human society was monogamous. If reason played a part in determining the nature of the human family unit, it is very probable that it was guided by the demands of man's omnivorous diet. The polygynous gorilla or baboon can guard his females from the attentions of other males while they forage together for fruits and young shoots. Primitive man, who, as his Palaeolithic arts display, was an animal largely dependent on a diet of meat, would not have gone hunting if in his absence his females were abducted by his fellows. Reason may have forced the compromise of monogamy."

I. Bergmann follows a German convention which seems to require the academic philosopher to write and lecture in a Dionysiac style. In the following translation of an average passage (pp. 130, 131) I have preserved the meaning, but have been unable to prevent the temperature from dropping several degrees below that of the original. I have relegated it to a footnote, because it seemed still too warm for presentation to a male scientific gathering. "And we shall always have to maintain that the tragic fundamental tone of the male's dramatic, ambiguous, exposed and precarious existence, based as it is on struggle and the elimination of his rivals, will forever prevent the emergence of a sane and happy communal life as displayed in the consummate victory of the maternal spirit, the joyous order and exuberant will to service of the social Hymenoptera. Of all this, indeed, little enough is to be seen in our modern social state, which is created and motivated by the awful splendor and grandeur of the male sexual tragedy, which manifests itself objectively in a perpetual war of the classes, in strife and masculine competition, and subjectively in the restlessness and conflict arising from the cravings and vital anxieties of the male sexual impulses, which are continually gnawing at the social order and hastening its break-up. Men created history, says

Mussolini. We answer: Certainly but what kind of a history! One written in blood and tears. Men created religion. Certainly, but what kind of a religion! Thousands of years of contemplation of the hereafter, inspired by worldly anxieties and dread of death. Men created the state. Certainly, but what kind of a state! A strange, misbegotten, anti-state, without vitality, a compromise-and-bastard state that follows neither the life plan of the sexes, nor a just apportionment of the sexual rôles, nor motherright. And the peculiar tragedy of the human race evidently lies in the fact that the female sex never will and never can create history, religion or a state. If the masculine spirit of intellect and leadership does not itself initiate a great change in human culture by transcending the constitutional male idiosyncrasies and by a true interpretation of the cultural trends of religion, morals and the state in obedience to the basic biological requirements, then, *finis humanitatis!* The signs of the times seem to point to a change but they can be correctly interpreted only with the aid of the key-science, which is the sociosophy of the sexes."

J. The insect societies might be called centripetal as contrasted with the centrifugal societies of vertebrates. The stability and high integration of the former are symbolized by the nest, which in all its forms — formicary, vespiary, apiary, termitary — is a communal *Gestalt*, a unitary, organized whole peculiar to the species, though constructed piecemeal by the sterile members of the colony. Somewhat similar structures are made by a few birds but not by mammals, because each female makes and occupies her own lair or burrow. Our cities with their separate family dwellings or apartments are not, therefore, strictly comparable with the nests of the social insects. This is true even of the communal houses of certain savages.

K. "The mother-family came naturally to be, by spontaneous growth out of antecedent conditions. The mother-clan persists indefinitely without any interruption, unless it dies out entirely. A father-family, on the other hand, necessarily breaks up every three or four generations at longest. The former is stable and

enduring, like the sex upon which it is based; the latter is active and variable, prone to movement, raiding and eventually to conquest. In its broadest features the mother-family is conservative, traditional and tends to equality in many respects, whereas the father-family is enterprising, progressive, sets free individual energy, and therefore promotes inequality.

"The limited communalism of the mother-family, chiefly as respects food, wastes capital where it does not prevent its accumulation; the energy of men is not stimulated. Its garden-culture by women is only a premonition of agriculture; tillage proper does not begin until men take it in hand. The mother-family has little history, because its character is a perpetuity of sameness. There is slight division of labor in it and therefore little societal organization. It is exogamy and the father-family which begin competition, combination, cooperation and organization. The evolutional movement which we call progress gains momentum with the father-family. War under the mother-family is caused by bickerings over emplacement and blood-revenge; captives are killed or tortured and only exceptionally adopted or enslaved. War has the character of raiding merely. In the father-family, war is less impulsive and is more organized and planned for a purpose by the authority on the ground, and is prosecuted more perseveringly. Its purpose is plunder and, at length, conquest, and its results subjugation, domination, enslavement and eventually the construction of territorial states. Slavery is the connecting link between the economic and militant forces in the evolution of society. Since family organization moves at the same time through the change which we are now viewing, the total organization of society undergoes a transformation which is difficult to embrace and understand with due allowance for all the elements in it." — W. G. Sumner and A. G. Keller, *The Science of Society* (New Haven: Yale University Press, 1927), III, 1984.